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ON THE EVOLUTION OF THE SOLAR SYSTEM

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1. *Introduction.*—For about a century it has been quite generally believed that, in an earlier state, our system was a widely extended nebula; and that, contracting under the mutual gravitation of its parts, rings were left off which later developed into the planets. There have been numerous variations from the original theory as formulated by Laplace, but they have all been essentially the same dynamically, for in all of them it was assumed that the original mass was in a state of temporary hydrodynamical equilibrium, maintaining its volume largely by gaseous expansion, and that the planets have developed out of rings left off from the parent mass. In 1900 Professor T. C. Chamberlin and the writer undertook, so far as was possible, to test,[1] by an appeal to the laws of dynamics, the consistency of this ring theory with known phenomena. Contradictions were uniformly found, and in some cases the results were so conclusive as to compel us frankly to abandon it as an untenable hypothesis.

Having given up the ring theory, the problem has been to find, if possible, something more satisfactory. The result has been the

[1] Chamberlin, "An Attempt to Test the Nebular Hypothesis by the Relations of Masses and Momenta," *Journal of Geology*, February-March, 1900. Moulton, "An Attempt to Test the Nebular Hypothesis by an Appeal to the Laws of Dynamics," *Astrophysical Journal*, **11**, 103–130, 1900.

formulation of a fairly definite theory, which Professor Chamberlin calls the "Planetesimal Hypothesis," and which he has expounded in his paper, "Fundamental Problems in Geology," in *Year Book* No. 3 of the Carnegie Institution of Washington, pp. 195–258. The present paper is devoted to a brief account of some of the main dynamical features of the theory, and to some comments on the retrograde revolution of *Saturn's* ninth satellite. For the sake of brevity in the exposition, the theory will be given in categorical terms, without implying in the least that it is not yet open to question at every point.

2. *Outline of the theory.*—It is supposed that our system has developed from a spiral nebula, perhaps something like those spiral nebulæ which Keeler showed are many times more numerous than all other kinds together.[1] The spiral nebula is supposed to have originated at a time when another sun passed very near our Sun. The dimensions of the nebula were maintained almost entirely by the orbital motions of the great number of small masses of which it was composed, and only a very little by gaseous expansion. It was never in a state of hydrodynamical equilibrium, and the loss of heat was not necessary for its development into planetary masses. The planets have been formed around primitive nuclei of considerable dimensions by the accretion of the vast amount of scattered material which was spread throughout the system.

Such a spiral nebula as that described, having originated in such a way, will develop into a system having the following properties: The planets will all revolve in the same direction, and approximately (though perhaps not exactly) in the same plane; the sun will rotate in the same direction, and nearly in the same plane, and will have an equatorial acceleration; the more the planets grow by the accretion of scattered matter, the more nearly circular will their orbits become; the planets will rotate in the forward direction, and approximately (though perhaps not exactly) in the planes of their orbits; the more a planet grows by the accretion of scattered matter, the more rapidly will it rotate; the planetary nuclei may be attended originally by many satellite nuclei revolving in any direction, but the scattered material will tend to drive all those satellite nuclei down

[1] *Astrophysical Journal*, 11, 347–348, 1900.

on to the primary nucleus which do not move forward in the general plane of the system; the scattered material develops and preserves circularity in the satellite orbits, if they revolve in the forward direction, but considerable eccentricity, if in the retrograde direction; a satellite may revolve more rapidly than its primary rotates; the system may contain many planetoids whose orbits are interlocked; the small planets will be cool and dense, and the large ones hot and rare; and the greater part of the moment of momentum of the system will belong to the planets. It will now be shown that these statements are true.

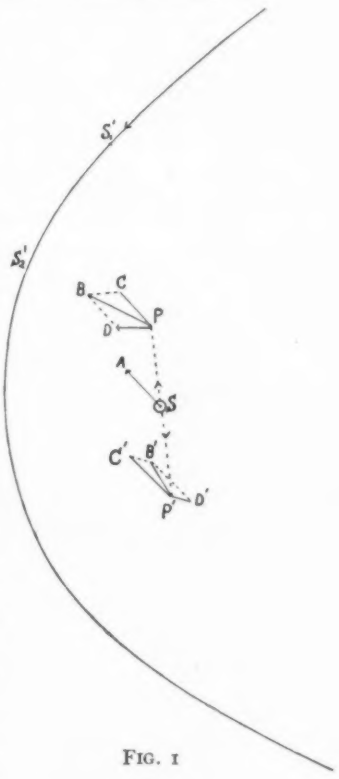


FIG. 1

3. *A possible origin of spiral nebulæ.*—In view of the relative motions of the stars, it is to be expected that two will sometimes pass near each other, and very much less frequently actually collide. At the time of near approach of two large masses the mutual tidal strains are very great. It is supposed that once a sun, which we shall call S', passed near our Sun, S, and raised on it a huge tide on the side toward S', and an almost equal one on the opposite side. It follows from the well-known theory of tidal forces that the effect on S was equivalent to a diminution of its attraction in the line passing through S', and an increase of its attractions in other directions. Roche has shown that when the bodies are nearer each other than 2.44 times their radii, the self-gravitation of one of them is more than balanced by the differential tidal forces due to the other. Our Sun was then agitated by great energies such as now produce the eruptive prominences. The enormous tidal strains attending the near approach of S' increased the eruptive tendencies of S toward and from S', and large quantities of material were ejected with great velocities in both of these directions. If it had not been for the subsequent disturbing effects of S', these ejected masses would have returned to the Sun, but S' drew them from their rectilinear paths and left them describing ellipses around the Sun.

To show how this was done consider Fig. 1. Suppose the masses

P and P' were ejected from S in opposite directions when S' was at S'_1, and consider the character of the disturbing forces when S' has arrived at S'_2. Let $\overline{SA}$ represent the acceleration of S' upon S in direction and amount. In the same units let $\overline{PB}$ and $\overline{P'B'}$ represent the acceleration of S' upon P and P', respectively. Resolve $\overline{PB}$ and $\overline{P'B'}$ each into two components, so that one of them in each case (PC and $P'C'$) shall be equal and parallel to $\overline{SA}$. Since $\overline{SA}$, $\overline{PC}$, and $\overline{P'C'}$ are equal and parallel, they do not disturb the relative positions of S, P, and P'. The disturbing accelerations are therefore $\overline{PD}$ and $\overline{P'D'}$ in direction and amount. It is observed that P and P' are both disturbed so as to start to revolve around S in the direction of the motion of S'.

While the character of the disturbing forces can be shown in this way, the precise results of their continued action can be determined only by computation. The labor of this computation is great, for the disturbing forces are very large, the curve described is complicated, and S' is for several years near enough sensibly to modify the motion of P and P'. The orbits described by the ejected material have depended upon the mass of S', the nearness of its approach to S, the relative position of S' at the time the material was ejected, and the velocity of ejection. At present it seems necessary in order to get a thorough understanding of the dynamics of the subject to treat by numerical processes a large number of special cases. This work is at present under way, and in all the cases so far considered P and P' have been left moving in elliptical orbits.

It remains to show that the nebula will have a spiral form immediately after the departure of S'. Figure 2 shows the positions of the masses ejected at successive intervals. The dotted lines are the actual curves which have been described, and the full lines show the apparent form of the spiral. There will be, of course, a vast quantity of fine material scattered throughout the system. The striking feature of the nebula is that there are two arms of the spiral starting from opposite sides of S. This is precisely the condition revealed by photography in the spiral nebulæ, often most unmistakably. It is to be observed that the motion is not along the lines of the spiral, and that, therefore, as the spiral grows older it will become more and more coiled.

Whether or not computation shall verify this conjecture respecting the origin and nature of spiral nebulæ, it seems probable, both from their appearance and from their spectra,[1] that the theory has led to a correct picture of their physical and dynamical condition. Doubtless those spirals which have been photographed are immensely larger than the one from which our system may have developed, and as a rule have relatively less massive centers.

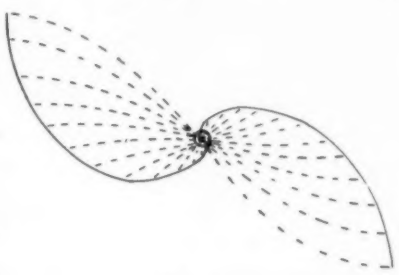


FIG. 2

4. *The revolutions of the planets.*—The matter was originally ejected more or less irregularly with occasional large nuclei which have grown into the planets by the accretion of the finer scattered material. The direction of ejection depended upon the direction of S', the lag of the tides on S, and the original direction and rate of rotation of S. It is very improbable that the plane of the original equator of S coincided with plane of the orbit of S'. Consequently the original ejections were not all exactly in the plane of motion of S'. A little consideration shows that, taking into account the ejections both toward and from S', and both before and after perihelion passage of S', the matter was distributed nearly symmetrically with respect to the plane of the orbit of S'.

It follows from the mode of generation of the elliptic orbits of the ejected material that the planets must all revolve in the same direction, and from the statements just made that their planes will nearly, though not exactly, coincide. It follows from the symmetrical distribution of the ejected material that the more a planet grows by the accretion of the scattered material, the more nearly will the plane of its orbit coincide with that of the orbit traversed by S'. Consequently we should expect but slight divergences in the planes of the orbits of the large planets, which have grown most, and on the average much greater differences in the orbits of such small bodies as *Mercury* and the planetoids. Interpreted according to this

[1] E. g., Scheiner found that the *Andromeda* nebula seems to have a dark line spectrum, *Astronomische Nachrichten*, **148**, 325, 1899.

theory, the high inclination (10°) of the orbit of *Eros*, which lies so close to the orbits of the Earth and *Mars*, is nothing to occasion surprise.

5. *The rotation of the Sun.*—The present rotation of the Sun is the resultant of its original rotation and of the disturbance due to S'. The original direction and rate of rotation of S are quite unknown. Its rotation was affected in two ways by the passage of S'. (*a*) S' raised large tides on the Sun and dragged them around in the direction of its motion. This contributed a certain rotation to the Sun in the direction of motion of S'. (*b*) Some of the ejected particles left the Sun with small velocities, and fell back upon it before their orbits were greatly perturbed by S'. But all of these particles had acquired some moment of momentum in the direction of the motion of S', and, falling obliquely into the Sun, they gave up their moment of momentum to this body. In these two ways a rotation was developed in the Sun agreeing approximately with that of the general motions of the planets.

Both of the influences which have been mentioned were most important in the equatorial zone, and extended to relatively shallow depths. Consequently the Sun was given an equatorial acceleration which still persists. The spots occur where the layers having different rates of rotation flow most rapidly past each other.

6. *The eccentricities of the orbits of the planets.*—The nuclei around which the planets formed were left by S' revolving in ellipses, presumably of considerable eccentricity. The probable amount will be revealed in the course of time by our computations. The orbits of the scattered particles had every possible orientation and crossed the orbits of the nuclei. The nuclei swept up these particles, and in the process had the eccentricities of their orbits changed. The question of interest is whether the eccentricities were increased or decreased. It is observed first that the more nearly two orbits have the same major axis and eccentricity, the more likely are bodies moving in them to collide; for under these circumstances the orbits may intersect at the most acute angle. The case of collision of two bodies moving in such orbits will be treated.

Let a_0 and e_0 be the common major semi-axis and eccentricity before collision, and a and e the corresponding elements of the orbit

of the combined mass after collision. Let N represent the mass of the nucleus, and m that of the particle. Let us neglect the slight perturbations that N and m produce in each other's motions, and consider only the effects of a collision at a point where their orbits cross. At the instant before the impact of the two bodies their kinetic energy was

$$\frac{1}{2}(Nv_N^2+mv_m^2) ,$$

where v_N and v_m represent their respective velocities. After impact the kinetic energy of their combined mass was

$$\frac{1}{2}(N+m)v^2 ,$$

where v is the velocity of the combined mass. Since part of the kinetic energy will have been transformed into heat by the impact, we have

$$Nv_N^2+mv_m^2>(N+m)v^2 . \tag{1}$$

But it follows from the theory of elliptic motion in the problem of two bodies that

$$v^2=k^2\left(\frac{2}{r}-\frac{1}{a}\right) . \tag{2}$$

Hence, since r was the same for both N and m the instant before impact, and for the combined mass the instant after impact, we have, substituting in equation (1),

$$Nk^2\left(\frac{2}{r}-\frac{1}{a_0}\right)+mk^2\left(\frac{2}{r}-\frac{1}{a_0}\right)>k^2(N+m)\left(\frac{2}{r}-\frac{1}{a}\right) .$$

It follows from this equation that

$$a<a_0 . \tag{3}$$

The moments of momentum of N and m before impact were respectively

$$k^2N\sqrt{a_0(1-e_0^2)} \text{ and } k^2m\sqrt{a_0(1-e_0^2)} .$$

After impact the moment of momentum of the combined mass was

$$k^2(N+m)\sqrt{a(1-e^2)} .$$

Since the moment of momentum of a system is not changed by collisions, we have

$$k^2N\sqrt{a_0(1-e_0^2)}+k^2m\sqrt{a_0(1-e_0^2)}=k^2(N+m)\sqrt{a(1-e^2)} .$$

Making use of the inequality (3), it follows that

$$e < e_0 \,. \tag{4}$$

That is, in this most important case the eccentricity of the nucleus was always decreased, in whatever manner the collision may have taken place. When the postulated conditions were anywhere nearly fulfilled, an overwhelming majority of collisions operated in the same way, though there were cases where the eccentricity was increased. The conclusion is that the more a planet grew by accretion, the more nearly circular, in general, its orbit became.

Let us compare this with the solar system. The orbits of the terrestrial planets average more than twice as eccentric as those of the great planets. Being nearer the Sun, where friction would have destroyed most irregularities, the ring theory would demand that their orbits should be more circular. The smallest planet, *Mercury*, has an orbit more than twice as eccentric as that of any other. The orbits of the planetoids are, on the average, three times as eccentric as those of the planets, and about one planetoid in four has an orbit more eccentric than that of *Mercury*.

7. *The rotation of the planets.*[1]—There is no reason to assume that the nuclei were originally rotating in any particular direction, and very probably they rotated in various directions. The present rotation of a planet depends upon the original rotation of its nucleus, upon the effects of S' on this nucleus, and upon the effects of the impacts of the scattered material. So far as S' influenced the rotation by the tides it generated, it tended to make it forward. The effects of the impacts will be considered, neglecting for simplicity the eccentricity of the orbit of the nucleus.

Suppose the nucleus N traveled around the Sun so as to fill the space between the curves a and b, and so that its center traveled along the circle c. The orbits of the small masses, m, which entered the path of N will be divided into three classes: (1) those orbits whose perihelia were inside of b and whose aphelia were between b and c; (2) those orbits whose perihelia were inside of c and whose aphelia were outside of c; (3) and those orbits whose perihelia were

[1] This idea was first developed briefly in approximately the present connection by Chamberlin, "A Group of Hypotheses Bearing on Climatic Changes," *Journal of Geology*, **5**, 653, 1897, footnote, pp. 668–669.

PLATE V

SPIRAL NEBULA, *M* 51 *CANUM VENATICORUM*
Photographed with Two-foot Reflector of Yerkes Observatory by G. W. Ritchey, 1902

between c and a and whose aphelia were outside of a. They are represented by the ellipses e_1, e_2, e_3, respectively in Fig. 3.

Case of e_1.—Since the major axis of the orbit of N was greater than that of m, it follows from equation (2) that at the time of their collision N overtook m. It is easily seen from Fig. 3 that this collision tended to give N a forward rotation.

Case of e_2.—In this case N and m were moving with nearly equal velocities, and it follows that the blow on N was largely radial. Therefore the effect of any single impact was small, and the combined effects of many, which tended to give rotations in various directions, cannot have been important.

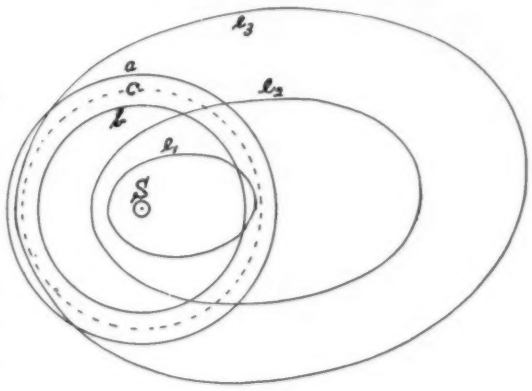


FIG. 3

Case of e_3.—In this case m overtook N outside of its center and tended to give it a forward rotation.

Thus, in the two cases where the impacts were most effective in changing the rotation, N was given a forward rotation. Therefore we should expect to find the planets rotating forward approximately in the planes of their orbits. The inclinations of the planes of their equators to the planes of their orbits might differ, because of differences in the original rotations of the nuclei, from planet to planet as in the case of *Saturn* and *Jupiter*. If there were any marked exception to the rule, we should expect to find it in the outermost planets where the particles moving in the orbits of the third type would be few. Moreover, the larger a nucleus was, the greater were the relative velocities of impact, the greater was the momentum contributed for a given mass and velocity, and therefore the larger was the amount of the moment of momentum contributed to N. For this reason, as well as for the fact that the larger nuclei probably acquired relatively larger amounts of scattered material than the smaller nuclei, we should expect to find the larger planets rotating more rapidly than the smaller.

8. *The satellites.*—When the planetary nuclei left the Sun, they were attended by many smaller secondary nuclei. When the velocity of the secondary nucleus was very small relatively to its primary nucleus, it was attracted into the larger mass and lost its separate existence. When the velocity of the secondary nucleus was large compared to its primary nucleus, it passed away from its gravitative control and began a career as an independent body. In all other cases the secondary nuclei revolved around the primary nuclei, and there is no reason for supposing they had originally any general direction of revolution.

For the purposes of the discussion, these secondary nuclei will be divided into three classes depending upon the positions of the planes of their orbits and their directions of revolution. The first class will consist of those whose orbits were highly inclined to the planes of motion of their respective primary nuclei; the second class, of those which revolved nearly in the planes of motion of the primary nuclei, but in the forward direction; and the third, of those whose revolution was nearly in the planes of motion of the primary nuclei and retrograde.

Secondary nuclei of the first class.—The problem for consideration is the effect of the impact of the scattered material with the secondary nuclei.

There is not room to go into details here, but it is easy to see that whenever the motion of the secondary nucleus was perpendicular to the plane of the general system, the scattered material acted upon it like a resisting medium. The result was that the size of the orbit was diminished. When its mass had become doubled by the accretion of the scattered material, its orbit had shrunk, by this cause alone, to one-fourth of its former dimension. In addition to this, the increase in mass of the planetary nucleus still further reduced its orbit. Consequently the secondary nuclei of the first class were in general precipitated on their respective primary nuclei. Only those have survived which were originally very remote, but not beyond the gravitative control of the primary, and which developed under the exceptional conditions prevailing near the borders of the system.

Secondary nuclei of the second class.—It can be shown, by a dis-

cussion similar to that given in section 7, that in this case the collisions tended to increase the dimensions of the orbits of secondary nuclei, and that, therefore, some of these bodies have maintained their separate existence. The increase of velocity was greatest when the secondary nuclei were remotest from their respective planetary nuclei. The principles of celestial mechanics show that this has made their orbits continually more nearly circular.

Secondary nuclei of the third class.—It can be shown, by a discussion similar to that given in section 7, that in this case the scattered material acted as a resisting medium. Moreover, up to a certain point the eccentricities were increased. Consequently most of those secondary nuclei which moved in the retrograde direction were precipitated upon the primary nuclei around which they revolved.

In a system having both direct and retrograde satellites it is to be expected, on the basis of this theory, that those moving in a retrograde direction will be very remote and have considerable eccentricities. This is the case with the ninth satellite of *Saturn*, whose orbit is about twice as eccentric as that of any other satellite.[1] The peculiar positions of the orbits of the satellites of *Uranus* seem to indicate that in this case the original secondary nuclei were moving in a special way. While the conditions prevailing in the satellite system of *Uranus* would not have been predicted on the basis of this theory, yet they do not definitely contradict it as they do the ring theory. The large planetary nuclei had gravitative control over secondary nuclei with large relative velocities, and for this reason the chances for their having several satellites are more favorable than in the case of small planets.

There is no reason why a satellite may not revolve more rapidly than its primary rotates, especially as its period continually diminishes as the mass of the planet increases.

9. *The planetoids.*—The planetoids have formed from material whose orbits did not cross a region swept by a large nucleus. Their ability to grow has been small because of their small dimensions and feeble gravitating power. The variations of the eccentricities

[1] According to the latest data, received since this statement was in print, the orbit of the seventh satellite of *Jupiter* seems to be retrograde and very eccentric.

and inclinations of their orbits are a measure of the heterogeneity prevailing after the passage of S' and before the equalizing effects of collisions had been realized. *Eros* is the one of considerable dimensions between the orbit of the Earth and *Mars*, whose orbit was so highly inclined that it did not lose its separate existence by collision with one of these larger bodies.

10. *The physical condition of the planets.*—The original nuclei which have grown into the terrestrial planets were of such small mass that they did not possess sufficient gravitative power to control true atmospheres. Consequently they cooled rapidly and became solid. The scattered material likewise lost its heat rapidly. Hence these planets have grown up from solid material, and have been solid throughout nearly their whole history since the visit of S', and their atmospheres have been acquired in a later stage of their evolution from the occluded gases which escaped as they contracted. The present internal heat and the past igneous action, of which there is abundant evidence in the Earth, are due partly to the residue of original heat which was not lost by radiation, and much more to the heat generated by the contraction of the planets from the density of the material when it fell on them to their present density. The writer has shown the quantitative effectiveness of such moderate contractions in bodies having the dimensions of the planets.[1]

The original nuclei which have grown into the great planets were large enough to retain true atmospheres. Consequently their original heat has been much more largely retained. For the same reason it is probable that they contain a larger proportion of the light volatile substances than the smaller planets do. The larger nuclei attracted the scattered material upon themselves with considerable velocities, and the heat generated by the impacts was very great. In the case of the small planets this surface heat was rapidly radiated away, but the atmospheres of the great planets retained it largely, and for this reason their original fluid state has been immensely prolonged.

11. *The moment of momentum of the system.*—It follows from the origin of the system that the remote planets should possess most of the moment of momentum of the system. This is precisely what is

[1] Chamberlin, "Hypotheses Bearing on Climatic Changes," *Journal of Geology*, **5**, 674, 1897, and Moulton, *Introduction to Celestial Mechanics*, p. 61, problem 5.

found. As Professor Chamberlin has pointed out, *Jupiter* contains about one-tenth of 1 per cent. of the mass within the orbit of *Saturn*, and more than 95 per cent. of the moment of momentum. This is an inevitable consequence of the spiral theory, but, on the contrary, the whole question of moment of momentum is a rock on which the ring theory breaks.[1]

12. *The ninth satellite of Saturn.*—The retrograde revolution of the ninth satellite of *Saturn*, taken in connection with the forward revolution of the other satellites and the direction of rotation of the planet, contradicts what has been considered up to the present time to be an inevitable consequence of the ring theory.[2] Nevertheless, the discoverer, Professor W. H. Pickering, has attempted to explain how it may have originated without contradicting the ring theory.[3] This section is devoted to an examination of these explanations.

Pickering's first suggestion is that *Phoebe* may have been originally a comet which has been captured by *Saturn*. It can scarcely be possible that the eccentricity of *Saturn's* orbit, or that the perturbations of *Saturn* by *Phoebe*, would assist in the supposed capture. Therefore we shall neglect in this discussion the eccentricity of the orbit of *Saturn* and the mass of *Phoebe*. This reduces the problem to that treated in the writer's *Introduction to Celestial Mechanics*, Chapter VII. It is shown there that if the constant of the Jacobian integral is sufficiently large, the surfaces of zero relative velocity are closed around the finite bodies, and consequently that a satellite or inferior planet in this case must remain permanently a satellite or inferior planet.[4] By the methods explained there it is found that if the constant is greater than 3.0180, the surface around *Saturn* is closed. The constant that belongs to the ninth satellite is found to

[1] Chamberlin, "An Attempt to Test the Nebular Hypothesis by the Relations of Masses and Momenta," *Journal of Geology*, **8**, 58–73, 1900. Moulton, "An Attempt to Test the Nebular Hypothesis by an Appeal to the Laws of Dynamics," *Astrophysical Journal*, **11**, 128, 1900.

[2] The observations at present seem to indicate that the sixth satellite of *Jupiter* revolves in the forward direction, and the seventh retrogrades at just about the same distance.

[3] *Annals of the Harvard College Observatory*, **53**, No. III, pp. 60–61.

[4] Hill used this method in connection with his special differential equations to prove the existence of a superior limit to the Moon's radius vector. *American Journal of Mathematics*, Vol. I.

have the value 3.0626. Consequently it can never escape from *Saturn's* control, and conversely it has never come under *Saturn's* control from a remote region. The radius of *Saturn's* largest closed surface of zero relative velocity is only about 40,000,000 miles, and it follows that *Jupiter* cannot have assisted in making the capture.

The other suggestion, which evidently was considered much more probable, is that *Saturn* once extended out to the orbit of *Phoebe*, and rotated in the retrograde direction with the angular velocity of the orbital motion of this satellite. The Sun is supposed to have raised large tides in this widely extended mass. Now, tides are generated on the side toward and the side opposite the tide-raising body, and it follows that the viscosity of the mass would in time reduce the rotation so that one side would be constantly toward the Sun. That is, tidal friction would in time give it a forward rotation with the angular velocity of the planet's motion around the Sun. As the planetary mass contracted, it would rotate more rapidly in the forward direction, and the interior nine satellites and the rings are supposed to have been left behind in this stage.

Let us examine the question quantitatively. Since the moment of momentum is a signed quantity, we may suppose that it is negative in the case of a retrograde rotation, and positive in the case of a direct rotation. Then, according to the explanation suggested by Pickering, the Saturnian nebula originally had a negative moment of momentum. The tides raised by the Sun destroyed this moment of momentum, and contributed positive moment of momentum until the rotation and the revolution of the mass were made in the same period. But when the rotation in the forward direction became faster than the revolution as the mass contracted, then the tides acted in the opposite direction and decreased the moment of momentum. Consequently, *the Saturnian system had its maximum moment of momentum with respect to the center of Saturn when its rotation was forward, with a period exactly equaling the period of Saturn's revolution around the Sun.* When the mass had shrunk down to the size of the orbit of *Japetus*, it must have rotated at the rate *Japetus* now moves, and we must find, if the theory is true, that its moment of momentum had been decreased by the retarding effects of the tides raised by the Sun.

Let us first compute an upper limit to the maximum moment of

momentum. We do not know the dimensions of the mass, except that it was somewhere between the orbits of *Phoebe* and *Japetus*, when it had its maximum. But the greater its radius was, the greater was its maximum moment of momentum. We shall certainly get something *too large*, if we suppose that this condition was realized when it extended entirely out to the orbit of *Phoebe*. We do not know the law of density, but the mass was certainly densest at its center. If we assume that it was homogeneous, we shall certainly get *too large* a result. Since the mass rotated on its axis but once in 29.5 years, it cannot have been sensibly flattened at its poles. Hence we shall certainly have an upper limit to the maximum moment of momentum, if we compute it under the assumptions that the mass extended out to the orbit of *Phoebe* at this time, and that it was homogeneous.

Consider the case when the mass had shrunk to the dimensions of the orbit of *Japetus*. It rotated in the period of revolution of this satellite, and may have been considerably flattened at the poles. If we assume that it was not flattened, we shall get a result which is *certainly too small*. If we assume that it is homogeneous, we shall get a result which is, so far as this factor is concerned, too large. We shall undoubtedly be nearer the truth if we assume that it obeyed the Laplacian law of density, and that its surface density was very small compared with the mean density.

With the assumptions made, the formula for the upper limit of the maximum moment of momentum is

$$M_M = \frac{2}{5} m R_P^2 \omega_S ,$$

where m is the mass of the Saturnian system inside of the orbit of *Phoebe*, R_P the mean radius of the orbit of *Phoebe*, and ω_S the mean angular velocity of *Saturn* around the Sun. The moment of momentum at the time the mass extended to the orbit of *Japetus* was less than, or perhaps about equal to,

$$M_J = \frac{2}{3}\left[1 - \frac{6}{\pi^2}\right] m R_J^2 \omega_J = 0.2614\, m R_J^2 \omega_J ,$$

where R_J represents the radius of the orbit of *Japetus* and ω_J is its angular rate of revolution.

Hence we find for the ratio of the upper limit of the maximum moment of momentum to that at the time the mass extended to the orbit of *Japetus*,

$$\frac{M_M}{M_J}=\frac{0.4R_P^2\omega_S}{0.2614R_J^2\omega_J}=\frac{1}{0.6535}\left(\frac{R_P}{R_J}\right)^2\left(\frac{T_J}{T_S}\right),$$

where T_J and T_S are the periods of *Japetus* around *Saturn* and *Saturn* around the Sun respectively. It is known that

$$R_J=2{,}225{,}000 \text{ miles},$$
$$T_J=79.3 \text{ days},$$
$$T_S=29.46\times 365.25 \text{ days},$$

and from Pickering's elements of the orbit of *Phoebe* that

$$R_P=7{,}996{,}000 \text{ miles}.$$

Consequently

$$\frac{M_M}{M_J}=0.1456 .$$

That is, we have found that the upper limit to the maximum moment of momentum is only one-seventh of something certainly less than the maximum. The only explanation of this contradiction is that the supposed development of the Saturnian system upon which the computation was based is erroneous. It follows that this retrograde revolution of *Phoebe* is actually, as well as apparently, squarely contradictory to the ring theory.

13. *Conclusions.*—While only abstracts of a portion of the discussions have been made in this paper,[1] enough has been said to show that the spiral theory is even now a good working hypothesis. It explains all the phenomena upon which the ring theory rested, and many others which are contradictory to the ring theory. Nothing has yet been found which seems seriously to question its validity.

The spiral theory raises a whole series of new and very difficult questions in celestial mechanics. These are the immediate effects of the tidal forces which are developed by the near approach of two suns, the perturbations of the orbits of matter which has been ejected by one of them under a variety of conditions, and the secular evolution of the orbits of this ejected material. A large amount of labor

[1] More details are given in the second volume of the *Geology* by Chamberlin and Salisbury, and in the writer's *Introduction to Astronomy*, both of which are to appear soon.

will be required to carry the discussion of these questions to a successful conclusion.

The spiral theory is fertile in suggesting new considerations for interpreting the immense variety of special phenomena of the system. It is not too much to expect that it may suggest new questions for observational investigation. It affords geologists new conceptions of the early history of the Earth. But perhaps its most interesting contribution is to our general philosophy of nature. Heretofore we have regarded the cosmical processes as forever aggregating matter into larger and still larger bodies, and dissipating energy more and more uniformly. Now we recognize important tendencies for the dispersion of matter.[1] This idea has introduced an element of possible cyclical character in the evolution of the heavenly bodies, though the question of the source of the requisite energy is serious.[2] There is hope that the difficulties of this question may soon be relieved, for recent discoveries respecting the internal energies of atoms suggest the possibility that the Helmholtzian contraction theory explains the origin of only a part of the energy given up by the stars.

UNIVERSITY OF CHICAGO,
August 30, 1905.

[1] This idea was first sharply worked out in Chamberlin's paper "On a Possible Function of Disruptive Approach in the Formation of Meteorites, Comets, and Nebulae," *Astrophysical Journal*, **14**, 17–40, 1901. The phenomena of radioactivity point in the same direction, though in quite a different way.

[2] This trouble is equally serious under the ring theory. See Chamberlin's "On Lord Kelvin's Address on the Age of the Earth as an Abode Fitted for Life," *Smithsonian Report* for 1899, p. 240.

VARIABILITY OF WAVE-LENGTH IN THE LINES OF SPARK SPECTRA

By NORTON A. KENT

HISTORICAL SURVEY AND STATEMENT OF THE OBJECT OF THE INVESTIGATION

In 1901 Haschek[1] stated that the lines of the spark spectrum of titanium exhibit considerable shift as compared with the same lines in the arc, a shift in one instance as great as 0.13 tenth-meter.

In 1903 the writer of the present paper made a careful experimental investigation of the subject.[2] No such large shifts were discovered; but it was shown that in all probability the wave-lengths of certain lines of the titanium spark spectrum are sensitive to various conditions of the electric circuit, such, for instance, as the amount of capacity, self-induction, and ohmic resistance employed, and the presence or absence of an auxiliary spark-gap in series with that under investigation.

The instrument used was a 10-foot Rowland grating. It seemed advisable to use greater dispersion. Hence the present work was undertaken with a 21-foot grating. During its progress, Eder and Valenta,[3] using a 15-foot instrument, have published a paper in which they state that in the case of neither pure zinc nor brass wire do the lines studied by Haschek show any shift. They attribute the results of these investigators to a photographic displacement of the center of gravity of the lines due to overexposure—the lines in question being those which exhibit very strongly the phenomenon of unsymmetrical broadening; and they suggest also the possibility of a pseudo-shift due to poor focus. The conclusion drawn by Eder and Valenta is that, at minimum photographic exposure, the coincidence of arc and spark lines is perfect. Moreover, Middlekauff[4] has lately studied the iron spectrum and finds no shifts in the region investigated. The focal length of his grating was 21.5 feet.

Such an inconsistency in the results of different investigators

[1] *Astrophysical Journal*, **14**, 181, 1901.
[2] *Ibid.*, **17**, 286, 1903.
[3] *Ibid.*, **19**, 251, 1904.
[4] *Ibid.*, **21**, 116, 1905.

emphasizes the importance of taking extreme care in all the details of the experiment, guarding against any possible false shifts, and stating with exactness the conditions under which the investigation is made.

The purpose of the present series of experiments is, then, to attempt to settle definitely the question of the coincidence or non-coincidence of arc and spark lines, to discover the determining conditions in case shifts do exist, and to harmonize, if possible, the results obtained by previous investigators.

EXPERIMENTAL CONDITIONS; CONSTRUCTION AND ADJUSTMENT OF THE GRATING-MOUNT

The conditions under which the present investigation has been carried out are as follows: The science hall of the college is a substantial building located not less than seventy-five yards from a roadway upon which there is heavy traffic; the foundations of the building are laid solidly upon glacial drift; and the room in which the spectroscope is mounted is situated in the cellar. The grating is a 21-foot Rowland concave, of 15,000 lines to the inch, having a ruled surface of 6 by $2\frac{1}{2}$ inches. The grating-holder is of brass, and is solidly built. The entire mount is of steel or iron. Seven-inch channel beams form two sides of the triangle; the third is a beam of 5 inches. The tracks are of $2\frac{3}{4}$ by $\frac{1}{2}$-inch rolled steel. The carriages are massive; the truss-rod connecting the two is of $2\frac{1}{4}$-inch piping; and three draw-bars conduce to great rigidity. The camera box is of iron and brass, designed by the author, and is especially massive. The photographic plate is held in its place by brass screws. The slit is connected rigidly to the channel beams, and the whole mount is supported by rubber disks on three substantial brick piers. After adjusting the instrument for any definite region of the spectrum, the bases of the grating-holder and camera box were always clamped to the steel tracks by nine iron clamps of various sizes.

The camera shutter, being supported from the floor, is entirely free from the camera box, and is so arranged that there is absolutely no chance of mechanical jar from the operation of changing the shutter between exposures. The slit mechanism is surrounded by cardboard cylinders, which are held free from the slit by supports

connected rigidly to the walls of the room. Around these cylinders is tied the black cloth which separates the dark spectrometer room from the light room in which are placed the spark and arc. The entire mount, including channel beams, steel tracks, trucks, truss-rod, draw-bars, and all possible parts of grating and camera box, is carefully wrapped in several layers of paper or cloth to facilitate obtaining constant temperature conditions. The steam pipes in the room are protected thoroughly by asbestos covering or several layers of paper.

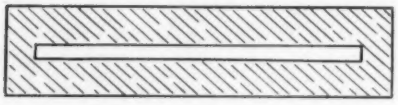


FIG. A

In adjusting the instrument the slit and grating rulings were made parallel by the use of a plumb bob. The coincidence of the normals to the grating and to the plane of the camera box was determined by using an illuminated slit 1 mm wide and adjusting until the image was horizontally coincident with the object. The ordinary method of superposing the image of a candle flame upon the flame itself was not regarded as sufficiently delicate.

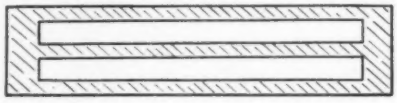


FIG. B

DURATION AND METHOD OF EXPOSURE

In general the exposures were short, ranging from three to ten minutes, and all the quantitative work of the investigation was carried on when temperature conditions were most favorable, the change in temperature as given by one or more thermometers whose bulbs were placed underneath the cover of the mount usually being absolutely unreadable, and never more than a half of a tenth of a degree Centigrade. Moreover, the method of exposure was such that pseudo-shifts, due either to mechanical jar or temperature change, must have been eliminated. The camera shutter was arranged as shown in the accompanying cuts. Usually the arc was exposed first, its spectrum being placed along the center of the photographic plate—this by use of the slit as given in Fig. A; then the spark image was thrown upon the slit of the spectroscope and its spectrum placed on the edges of the plate—this by the use of the slits shown in Fig. B; then

FIG. C

the first operation was repeated, and the arc spectrum again thrown upon the center. But in most cases, always during the latter part of the investigation, the shutter was so arranged that the vertical superposition of the first and third exposures was not perfect, so that the central portion of the plate containing the arc spectrum presents some such appearance as that shown in Fig. C. A mechanical shift may thus be easily detected.

PRODUCTION OF THE SOURCES OF LIGHT

The spark was produced in the following manner: The city current, of 110 volts and 133 cycles per second, was led into a 5-kilowatt transformer (110 to 15,000, 30,000 or 60,000 volts) manufactured by the Central Laboratory Supply Co., of Lafayette, Ind., in series with which was a home-made water rheostat. The secondary voltage ranged from approximately 1600 to 14,000 volts, as the ratio of transformation which was employed was 30,000 to 110. The high potential current was led into glass-paraffin-tin condensers giving a range of capacity varying from about 0.008 to 0.08 microfarads (see Table I). Both the charging and discharging high potential wires were very heavy (about No. 8 standard wire gauge).

TABLE I

CAPACITY OF CONDENSERS

No. of Condenser	Approximate Area of One Element in cm²	Mean Thickness of Glass in cm	No. of Elements	Total Capacity in Microfarads
1	900	0.245	4	0.0076
2	900	0.249	12	0.0226
3	900	0.241	10	0.0195
4	730	0.246	20	0.0315
5	730	0.479	8	0.0067

The self-induction coil employed consisted of three concentric cylindrical coils of dimensions given below (see Table II). Generally, when photographing the spark, there was placed in the rear of it, at a distance of 80 cm, a 12-inch Westinghouse fan, the strong air current from which was directed upon the spark-gap by a megaphone 70 cm long, the smaller opening (4 cm in diameter) being about 2 cm distant from the spark. When a secondary spark was used in series with that under investigation, the side draft from the fan was allowed

to play upon it also. The fan undoubtedly produces greater disruptiveness and increases the potential of the discharge; but its steadying influence is very desirable, especially when self-induction is used in the discharge circuit. The luminous image was formed on the slit by a glass lens (aperture 3 inches, focal length 25 inches, made by Bausch & Lomb) situated about 110 cm from the source of light and 152 cm from the slit. This lens was held in a metal casing clamped to a marble slab which was set on an iron pipe imbedded in a cement foundation.

TABLE II

DIMENSIONS OF SELF-INDUCTION COIL IN CM. GAUGE NUMBER OF WIRE = 14 S.W.G.; EXTERNAL DIAMETER WITH INSULATION = 0.54 CM

Coil	Length	External Diameter	Total No. of Turns
Inner	66.0	5.22	124
Central	65.3	8.59	122
Outer	64.7	9.95	122

In all cases the spark which was being photographed was placed in a horizontal position, in view of the possibility that different regions of the spark might show different spectra.

In the primary circuit of the transformer were placed a Thomson indicating watt-meter of maximum voltage 150 volts and maximum current 50 amperes, graduated in hectowatts, and a Thomson alternating ammeter of maximum current 100 amperes. The primary voltage was read by a Weston direct reading, alternating and direct current voltmeter, a two-scale instrument, 15 and $7\frac{1}{2}$ volts, with 1 to 10 multiplier.

The arc was developed by a 133-cycle alternating circuit, the current used being about 12 amperes, measured by a 50-ampere Ayrton and Perry spring ammeter. The discharge took place between one-half inch carbons, the lower one of which was of the cored type and was hollowed out a trifle to receive the small pieces of metal used. The distance between the terminals was always in the neighborhood of 3 mm. That the energy for the spark and arc discharges could be obtained from the city circuit was a distinct advantage, for under these conditions there was no possibility of mechanical vibration due to the presence of heavy electrical machinery.

REDUCTION OF ASTIGMATISM, METHOD OF MEASURING PLATES, ETC.

The investigation was confined to the second spectrum, the dispersion being one Ångström unit to 0.75 mm. The length of the rulings of the grating surface was diminished by reducing the vertical aperture to about 2 cm (using black paper for the purpose); this process, together with stopping down the length of the slit to 5 or 6 mm, so much reduced the astigmatism that the tips of the spectral lines could be seen in the field of the micrometer used in measurement. We thus avoid a shift due to overexposure, for, by setting the cross-hair upon the tip of the spark image, where it is just visible to the eye, we are certainly making use of minimum exposure conditions.

The filar micrometer, manufactured by Bausch & Lomb, was reinforced by adjustable trusses to produce greater rigidity. A test of the accuracy of the instrument showed that the errors of measurement were greater than those of the screw. The magnification employed was about seven diameters, and a single cross-hair was found to give the best results. The plates were placed violet left, and four settings were made on the external or spark spectrum, then two upon the internal or arc spectrum; then the same set of measurements was repeated with the violet to the right. The plates used were the regular Seed "Gilt Edge" 27, 5×7, cut into strips $1\frac{1}{4}\times 7$ inches.

The developer generally used was that described by Wallace,[1] as giving with the Seed 27 plate a definite and regular silver grain deposit.

Several plates were developed with Jewell's solution,[2] to which was added an excess of bromide (roughly 0.5 gram of potassium bromide to a 3 oz. solution).

[1] *Astrophysical Journal*, **20**, 118, 1904. The formula was kindly sent the author by Mr. Wallace, and is as follows:

	Grams		Grams
A. Hydrochinon	1	B. KBr	1
Metol	1	$NaOH$	12
Adurol	2	H_2O	175
Na_2SO_3 (crystals)	24		
H_2O	175		

Take equal parts of A and B.

	Grams		Grams		Grams
A. Hydrochinon	30	B. Potas. ferrocyanide	30	C. K_2CO_3	30
Na_2SO_3 (crystals)	150	H_2O	180	H_2O	180
H_2O	750				
Alcohol	10 cc.				

Take one part of B, one of C, and six of A. Normal amount of bromide, 15 drops (10 per cent. solution) to 100 cu. cm of developer.

RESULTS

The results are contained in the following tables, concerning the data given in which a few general remarks may be made:

The time of the two arc exposures varied from 10 to 60 seconds, the short exposures predominating.

The length of the slit, unless otherwise stated, was either 5 or 6 mm, its width between 0.025 and 0.050 mm.

The arc spectrum was generally thrown on the center of the plate.

The values of current, power, and voltage given in the tables are the means of from two to six (average three) readings taken at fairly regular intervals during the exposures. The conditions of the circuit were quite stable, the three elements seldom varying more than 1 ampere, 20 watts, and 0.3 volts respectively.

The metals used were: titanium carbide or "cast titanium," made by Eimer & Amend of New York, 85 per cent. titanium, 15 per cent. carbon; a titanium-iron alloy, 20 per cent. titanium, 80 per cent. iron (Eimer & Amend); zinc battery rods; brass wire, 30.2 per cent. zinc, 69.8 per cent. copper; ordinary iron rod.

Unless otherwise stated, the part of the spark image used was that near one terminal, and the fan was employed to steady the discharge.

The secondary spark took place between brass rods 4 mm in diameter.

The focus, to an accuracy of 1 mm, was determined repeatedly from week to week as the temperature of the room changed. This was a precautionary measure: no change in focus was noticed.

The given values of the shifts represent generally but one set of measurements on each plate, never more than two. This course was pursued because the different plates agreed so well that numerous measurements upon the same plate were deemed unnecessary. The average deviation from the mean of the measurements on the different plates in all the sets was about 0.0027 tenth-meters for titanium and iron, and 0.0070 for zinc. The average deviation from the mean of different measurements of a line on any one plate was about 0.0025 tenth-meters for titanium, 0.0018 for iron, and 0.0060 for zinc.

To gain in another manner an idea of the degree of accuracy of

PLATE VI

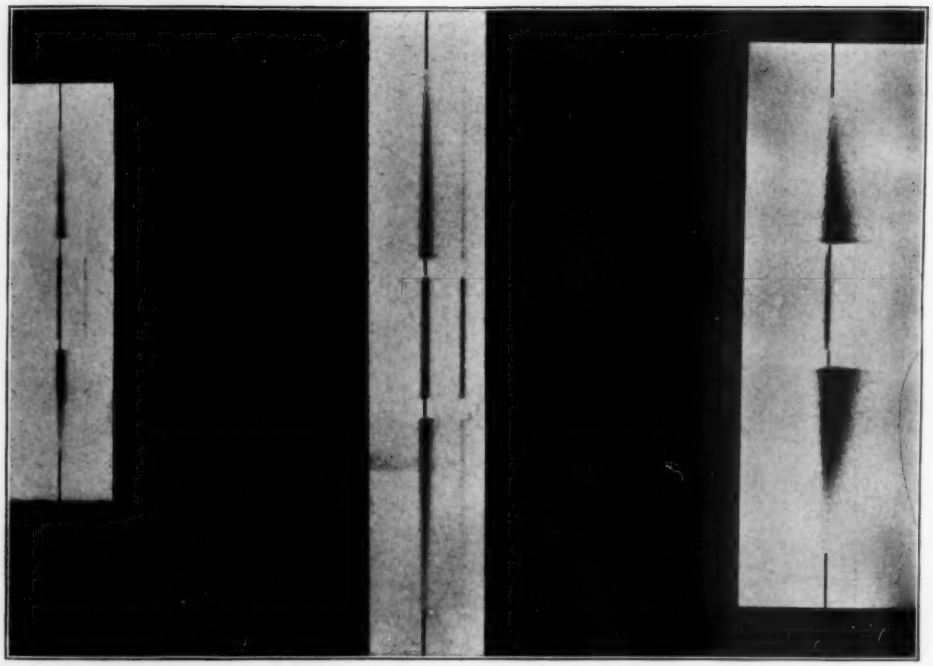


FIG. 1 FIG. 2 FIG. 3

FIG. 1.—Negative of *Ti* λ 3913.58 as apparent on plate No. 22. Mean shift as given by plates Nos. 22 to 25, 0.031 t.-m. Scale of enlargement about 5 : 1.

FIG. 2.—Negative of *Ti* λ 3913.58 as apparent on plate No. 27. Influence of self-induction shown. Mean shift as given by plates Nos. 26 and 27, 0.005 t.-m. Scale of enlargement about 7 : 1.

FIG. 3.—Negative of *Zn* λ 4722.26 as apparent on plate No. 47. Mean shift as given by plates Nos. 47, 48, and 43, 0.065 t.-m. Scale of enlargement about 5.5 : 1.

measurement, four settings, two red-right and two red-left, were made[1] on each of three lines which appeared on a focus plate: *Ti*, λλ 3898.645, 3901.114, and 3904.926, as given in Rowland's table of solar wave-lengths. The first line only was reversed; all were somewhat too dense for very accurate settings. Assuming the values of the wave-lengths of the first and third lines as given, that of the second was calculated. The first determination gave 3901.112, the second 3901.114; the mean being 3901.113, but 0.001 tenth-meter different from the wave-length as given above.

The spark lines which were studied most thoroughly—namely, *Ti*, λλ 3900 and 3913; *Zn*, λλ 4680, 4722, and 4810—generally appear enhanced, diffuse, and unsymmetrically broadened. Many iron lines also show unsymmetrical broadening to some extent.

Figures 1, 2, and 3 of the accompanying plate are illustrative of the lines studied. The scratches, appearing at the tips of the spark and between the arc and spark segments, were drawn by pricking two holes in the film of a positive of an original negative at points which seemed to lie in the "center of gravity" of the very tips of the spark lines. These two points were then joined by a ruler and the film carefully scratched away along its edge in the four regions above mentioned. A negative of the positive was then made.

The arc line lies considerably to the left, or violet, in all cases but that of Fig. 2, which is a "self-induction line." It will be noticed that the double arc exposure sketched in Fig. C appears only in Fig. 3 of the plate. As stated previously, this non-superposition method was not used at the very first of the investigation.

Let us discuss first the results as given by the titanium plates in Table III:

1. The members of the set of six plates, set 1 (see next to last column of the table), agree in showing that with large capacity shifts exist (about 0.023 tenth-meters), even in the absence of a secondary gap.

2. Set 2 (5 plates) shows that, with a secondary gap of 4 mm and lessened capacity, considerable shifts exist (about 0.037 tenth-meters) The effect of a secondary gap is to increase the potential of the discharge, and this gives the same result as an increase of capacity.

[1] A suggestion due to Professor Frost.

TABLE III

SHIFT OF SPARK LINE TOWARD RED IN

No. of Plate	Date	Length of Spark Exposure in Minutes	Constants of Electric Circuit						Character of Lines S = Sharp D = Diffuse
			Primary			Secondary			
			Amps.	Watts	Volts	Capacity in Microfarads	Length of Gap in mm	Part of Image Used	
							Primary / Sec'dary		
6	Dec. 15....	15	40.6	193	6.8	0.0812			
7	" 15....	15	41.1	180	7.0	0.0812			
8	" 16....	15	40.4	188	6.5	0.0812	1.0 / 0	End	D
9	" 16....	22	40.9	172	5.8	0.0812			
10	" 17....	26	39.7	188	6.3	0.0812			
28	" 17....	15	40.0	160	7.2	0.0803			
12			39.0	390	12.0		1.5		
13			37.8	390	12.5		2.0		
14	" 17....	5	38.3	400	13.0	0.0195	1.7 / 4.0	End	D
15			38.5	380	11.3		2.1		
16			38.5	380	12.2		2.5		
22	Jan. 14....	6	35.5	1010	46.0		2.0 / 31		
23	" 17....	5	34.1	910	37.0	0.0067	1.0 / 27	End	D
24	" 17....	6	34.4	910	36.0		2.0 / 27		
25	" 17....	5	34.0	883	38.3		2.0 / 27		
26	" 17....	5	35.0	717	35.0	0.0067	2.0 / 27	End	S
27	" 21....	7	33.6	775	41.0		/ 25		
70	April 15....	5	41.5	415	11.6				
72	" 15....	3	42.4	400	11.3	0.0226	2.0 / 5	End	D
93	May 5....	8	41.4	395	10.9				
99	" 5....	11	41.2	400	11.2				
77	April 15....	5	41.7	397	11.1	0.0226	2.0 / 5	End	S
78	" 15....	9	41.2	420	11.3				
73	" 15....	4	41.5	440	12.0				
74	" 15....	3	41.1	410	11.5				
97	May 6....	9	39.8	417	11.2	0.0226	9.0 / 0	End	D
101	" 6....	10	41.2	410	11.5				
102	" 6....	10	41.0	427	11.5				
75	April 15....	4	41.6	425	11.7				
76	" 15....	3	41.5	435	11.5	0.0226	9.0 / 0	Center	D
96	May 5....	6	41.5	425	11.4				
98	" 6....	9	41.0	425	11.1				
80	April 29....	10	41.3	297	9.1				
81	" 29....	15	41.9	313	9.0	0.0226	4.0 / 0	End	S
82	" 29....	15	41.9	313	9.0				
83	" 29....	5	42.1	320	8.9				
84	" 29....	4	41.5	315	9.0				
87	May 2....	8	41.8	300	9.2				
88	" 2....	7	42.0	300	9.1	0.0226	4.0 / 0	End	D
103	" 10....	13	40.7	307	8.7				
104	" 10....	10	40.2	300	8.5				
105	" 10....	8	40.9	315	8.6				
106	" 10....	8	41.3	310	8.4				

TITANIUM

TENTH-METERS: POSITIVE UNLESS OTHERWISE STATED

Metal Used	Fan	Shift				No. of Set	Remarks
		λ 3900.68		λ 3913.58			
		Separate Measurements	Mean	Separate Measurements	Mean		
85% *Ti* 15% *C*	Yes	0.036*	0.023	0.031	0.024	1	Plate No. 8, spark inside; arc outside
		25		24			
		21		20			
		18*		22			
		21		24			
		18		22			
85% *Ti* 15% *C*	Yes	0.042	0.038	0.048	0.036	2	Plate No. 16, spark inside; arc outside
		35		30			
		38		39			
		37		31			
		39		34			
85% *Ti* 15% *C*	Yes	0.045	0.035	0.041	0.031	3	Plate No. 25, spark inside; arc outside
		36		25			
		26		28			
		32		30			
85% *Ti* 15% *C*	Yes	0.002	0.005	0.004	0.005	4	Triple coil of self-induction used
		8		7			
85% *Ti* 15% *C*	Yes	0.027*	0.020	0.026*	0.022	5	Developed with Jewell solution
		15*		23			
		21		16*			
		17		24			
20% *Ti* 80% *Fe*	Yes	0.001	0.002	0.003	0.003	6	
		4					
85% *Ti* 15% *C*	No	0.020	0.019	0.017*	0.018	7	Developed with Jewell solution
		14*		17*			
		22		19			
		21		23			
		17		15			
85% *Ti* 15% *C*	No	0.006	0.006	0.003	0.005	8	Developed with Jewell solution
		6		6			
		6		5			
		7		7			
85% *Ti* 15% *C*	No	—0.001	0.001	—0.001	—0.001	9	Two meters of fine wire, resistance 2.7 ohms, in discharge circuit of condenser
		— 7		— 6			
		10		3			
85% *Ti* 15% *C*	No	0.020*	0.017	0.023*	0.017	10	Developed with Jewell solution
		18*		18*			
		16*		12*			
		14*		15*			
		13		15			
		20		20			
		18		17			
		14		17			

* Mean of two measurements.

3. A condition (set 3) given by an increase of secondary gap and further reduction of capacity still presents a shift (about 0.033 tenth-meters). It may also be seen that plates Nos. 8, 16, and 25 (sets 1, 2, and 3), on which the arc spectrum was photographed outside of the spark spectrum, show no marked or consistent difference from those obtained in the ordinary manner. Care was taken that the spark exposure be short.

4. Introducing the self-induction coil into the discharge circuit of the condenser (set 4) unquestionably conduces to coincidence of arc and spark line (mean value of shift 0.005 tenth-meters).

5. Sets 5 and 6 show that the use of the alloy, 20 per cent. titanium, 80 per cent. iron, gives much less displacement (0.002 tenth-meter) than that given by titanium carbide, 85 per cent. titanium, 15 per cent. carbon (about 0.02 tenth-meter).

6. The part of the spark image used is extremely important; set 7, given by that region of the image near the terminal, shows a shift of 0.018 tenth-meter, while the central portion of the spark (set 8) 4.5 mm from the terminal shows but about 0.006 tenth-meter.

7. Sets 9 and 10 show that ohmic resistance (2.7 ohms) in the discharge circuit of the condenser produces an effect exactly similar to that of self-induction (see result No. 4), reducing the shift from about 0.017 tenth-meter to practically zero.

8. Sets 7 to 10, in which no fan was used, show that the same is not a *sine qua non* in the matter of shift.

9. Further, the developer used appears to have no effect upon the shift. Therefore in sets 5, 7, 8, and 10 the measurements made on plates developed with the Jewell solution are averaged in with the others. Indeed, it is difficult to see how there could be a difference, provided the tip of the line be used in measurement. If, however, the setting be made upon the body of the line, a difference might very easily be introduced (see below, result No. 14), a "softer" developer, such as Wallace's, tending to displace the apparent center of gravity of the line toward the side of greater broadening.

Passing to zinc, Table IV, we have:

10. Sets 11, 12 and 13 give results comparable with those of 3, 1, and 2 for titanium, and sets 14 and 15 confirm the alloy effect of 6 and 7.

TABLE IV. ZINC

SHIFT OF SPARK LINE TOWARD RED IN TENTH-METERS

No. of Plate	Date	Length of Spark Exposure in Minutes	Constants of Electric Circuit: Primary: Amps.	Watts	Volts	Secondary: Capacity in Microfarads	Length of Gap in mm.: Pri.	Sec.	Part of Image Used	Shape of Terminals	Metal Used	Fan	Shift: λ 4680.38 Separate Measurements	Mean	λ 4722.26 Separate Measurements	Mean	λ 4810.71 Separate Measurements	Mean	No. of Set
31	Jan. 27	10	33.4	955	36.0		?	25					.....		0.066		0.048		
33	Jan 28	10	32.5	946	38.8		2	24		Flat			0.019		43		30		
34	Jan. 28	15	32.8	965	41.0	0.0067	2	21	End	3×6 mm	*Zn* battery rods	Yes	37	*0.028*	40	*0.042*	30	*0.039*	11
36	Feb. 3	15	34.8	983	41.0		2	21					24		36		48		
37	Feb. 3	15	33.2	993	42.5		2	21					32		26		..		
38	Feb. 3	30	41.1	193	7.4	0.0736	1	0	End	"	"	"		*0.025*		*0.047*		*0.039*	12
41	Mar. 4	4	41.0	440	12.8	0.0195	1.5	3	End	Round 3 mm diam.	"	"	0.034		0.047		0.071		
42	Mar. 4	6	40.8	445	13.2								33	*0.033*	48	*0.047*	59	*0.065*	13
45	Mar. 11	20	41.2	343	9.6	0.0226	2	2.5	End	"		"	0.003		0.000		0.017		
46	Mar. 11	18	41.7	350	9.7					Flat 1×3 mm	30.2% *Zn* 69.8% *Cu*		.....	*0.003*	6	*0.003*	8	*0.012*	14
47	Mar. 11	10	40.7	490	14.2	0.0226	2	2	End	"	*Zn* battery rods	"	0.029*		0.059*		0.084*		
48	Mar. 11	10	40.8	463	13.8			2					.....	*0.032*	63	*0.065*	125	*0.102*	15
43	Mar. 4	5	42.0	385	11.1			2.5					35		74		97		

Set 14: Filings from zinc battery rod used in arc as in other cases. Lines sharp.
Plate No. 48 developed with Jewell solution and normal amount of bromide.
* Mean of two measurements.

In the case of iron, Table V, we find:

11. Among the lines investigated, the shifts are in general very small, but are nevertheless present.

Further facts are as follows:

12. A plate on which three exposures were made in the usual manner, the first and third being superposed, and the source of light being the titanium arc, shows that no shift is introduced by throwing a diffuse image on the slit, destroying the focus by moving the arc

TABLE V. IRON[1]

SHIFT OF SPARK LINE TOWARD RED IN TENTH-METERS

Wave-Length	Shift	Wave-Length	Shift
3688.58	0.004	3788.01	0.001
3705.10	2	95.13	3
09.37	3	99.68	3
20.07	3	3815.97	10
27.78	3	27.96	6
48.39	5	34.37	7
58.36	5	40.58	2
63.93	6	41.19	8
67.34	5		

two inches nearer the lens. This proves that even if either arc or spark were out of focus to a small extent, the shift would not be altered.

13. A plate exposed to the carbon and graphite arcs shows that no shift occurs between lines which are due to the same impurities in both. The currents used were 33 and 75 amperes respectively. The graphite arc is produced with considerable difficulty, a large current being required, and the discharge is quite explosive. It was thought that this explosive effect might produce a small shift toward the red.[2] Evidently the disruptive effect is of too great a period and not sufficiently violent.

14. A plate was taken under conditions similar to those of Nos.

[1] The conditions under which the iron plates were obtained are as follows: six plates taken at various times; average spark exposure, 12 minutes; average current, power, and voltage, 41.3 amperes, 420 watts, 11.5 volts; capacity, 0.0226 microfarads; length of primary gap, 9 mm; no secondary gap and no fan; region of spark image used, that near terminal. The above values of the shift are means of measurements made on from two to six plates.

[2] The suggestion that a comparison of these two spectra be made is due to Professor Crew.

83 and 84, except that the slit-length was extended to about 1 cm, the whole vertical aperture of the grating was used, and the exposure lengthened to ten minutes. Under these conditions there results a greatly overexposed spark spectrum. Upon careful measurement, setting on what appears to be the maximum of intensity of the body of the line, shifts as great as 0.031 and 0.025 tenth-meter appear; whereas set 10 gives values of 0.017 and 0.017. It is evident, then,

TABLE VI. TITANIUM

DATA TRANSFERRED FROM TABLE III

Mean Current in Amperes	Mean Power in Watts	Mean Potential Difference in Volts	Mean Shifts for λλ 3900, 3913	Set	Effect of
40.5	180	6.6	0.023	1	
38.4	388	12.5	37	2	
34.5	928	39.3	33	3	Self-induction
34.3	746	38.0	5	4	
41.6	402	11.3	21	5	Alloy
41.5	409	11.2	2	6	
40.9	421	11.5	18	7	Part of spark image
41.4	428	11.4	5	8	
41.7	308	9.0	0	9	Ohmic resistance
41.3	308	8.8	17	10	

as Eder and Valenta suggest, that by overexposure a false displacement may be introduced in a line marked by unsymmetrical broadening.

15. To confirm the data given in Table III, which concern only the lines *Ti*, λλ 3900, 3913, a series of plates was taken in other regions of the titanium spectrum. A study of the lines λλ 4443.97, 4468.65, 4489.24, 4533.42, 4534.15, 4549.79, 4563.94, and 4572.15 showed the presence of the alloy and self-induction effects of sets 6 and 7, also 3 and 4, Table III, respectively.

It was found also that large shifts are developed in other lines than λλ 3900, and 3913, namely, 0.024 tenth-meter and 0.036 tenth-meter for λ 4827.74 and λ 4836.25 respectively.

Further, when both spark and arc lines appear reversed, the central portions of the reversals are coincident (e. g., λλ 3223.1 and 3237.5).[1]

16. By referring to Table VI, the data given in which are taken

[1] Approximate wave-lengths.

from Table III, it will be seen that the energy involved is not the only element in the shift, for sets 3 and 4, in which nearly the same power (928 and 746 watts) was used, show a large difference in shift. This is due to self-induction, which causes less disruptiveness in the discharge.

Ohmic resistance (sets 9 and 10) gives the same effect.

The influence of a low-percentage alloy appears in sets 5 and 6, and the difference between the center and end regions of the spark is shown in sets 7 and 8.

It may be remarked that set 3 (power 928 watts) shows less shift, 0.033, than set 2 (power 388 watts), 0.037; but the difference is very small, and, moreover, the disruptiveness in the latter case may have been greater.

SUMMATION OF RESULTS

In conclusion it may be said that the writer's former results have been qualitatively and, to a certain degree, quantitatively confirmed. It is difficult to see how we can avoid the following deduction: Under certain conditions, namely, when self-induction and ohmic resistance are absent, large capacity or long secondary gap are used, and the terminals are of a high-percentage alloy, the part of the spark which lies near the terminal gives a spectrum, the wave-lengths of the lines of which are greater than those of the arc spectrum. The shifts occur when the energy involved in each discharge is great, and the discharge is disruptive in nature. These conditions mean the creation of a large amount of metallic vapor in a short time—a highly explosive phenomenon. That an increase of wave-length should result is rather to be expected. This may be rendered more evident from the following discussion:

In addition to those theoretical considerations (mentioned in the writer's former paper)[1] which would lead us to expect an increase in wave-length, namely those dealing with the pressure, p, in a gaseous medium as a function of the number of particles per c.cm, n, the mass of each, m, and the mean squared velocity, u^2, postulating the truth of the equation $p=\frac{1}{3}\, m\, n\, u^2$, it may be stated that such an increase in pressure may come from the fact that, at the beginning of the discharge, the air acts as an incompressible medium (compare the downward explosive effect of dynamite placed on the surface of

[1] *Astrophysical Journal*, **17**, 296, 1903.

the Earth). The development of the metallic vapor takes place so quickly that the air has no time to move. The lessened shift in the center of the spark can then be easily explained, for it is known that some time elapses before the metallic particles, advancing from the terminal, reach the central region of the spark. When they have finally reached this region, the air has yielded and the pressure fallen. An unusual amount of vapor is disengaged in the discharge between electrodes of titanium and zinc in water (not so prominently the case with iron). That the pressure developed is also great was noticed by the writer in his work with Professor Hale on the spark under water.[1] The terminals were badly shattered; indeed, there is good reason to expect that the lines of the spectrum given by the discharge in air pass through a series of changes which resembles that in water. Certain titanium lines appear reversed on some of the present series of plates, and not so on others. The reversal comes up unsymmetrically on the violet edge of the center of gravity of the line, a common occurrence in the spectra of the spark under water.

POSSIBLE EXPLANATION OF THE INCONSISTENT RESULTS OF HASCHEK, EDER AND VALENTA, AND MIDDLEKAUFF

Haschek undoubtedly dealt with overexposed lines and increased the true shift by a photographic displacement of the region of maximum intensity.

Eder and Valenta probably used a vertical spark and set the central region of the same on the slit of the spectroscope. Attention is called to the fact that the lines studied by the writer, *Zn*, λλ 4680, 4722, and 4810, are among those investigated by Eder and Valenta. We are assuming that the same effect obtains in the case of zinc as with titanium (sets 7 and 8, deduction No. 6 above) with reference to the central and end portions of the spark. That no shift exists under the conditions employed between the absorption regions of reversed arc and spark lines, as stated by Eder and Valenta for zinc, was found to be true in the case of titanium.

Lastly, that Middlekauff found no shift with iron may easily be explained by the fact that few lines show a shift, and these generally in but a small degree, under highly disruptive conditions and near the region of the terminal. Middlekauff probably used a compara-

[1] *Astrophysical Journal*, **17**, 154, 1903.

tively weak spark, set its image vertically on the slit, and integrated with his spectrometer the spectra of the central regions.[1] Moreover, Middlekauff and Eder and Valenta may have used small wires in the discharge circuit of the condenser and so have introduced ohmic resistance.

INFLUENCE OF RESULTS UPON THE MAPPING OF SPARK SPECTRA AND UPON STELLAR LINE-OF-SIGHT WORK

The bearing of the above results upon radial-velocity investigations in which a titanium spark is employed to give the comparison spectrum is such as to emphasize the fact that low-energy and non-disruptive conditions should be employed, if the lines of the resultant spectra are assumed to be severally of the same wave-length as those of the arc or the absorption and bright lines of the stars. Furthermore, if a highly disruptive spark be used, the same circuit conditions should be employed at all times in any one observatory, to make the plates there taken mutually comparable; or, finally, the same conditions should prevail at all observatories if mutually consistent results are desired.

The general bearing upon the mapping of spark spectra may be seen in the fact that an agreement between the wave-lengths of spark lines as measured by various investigators is impossible unless the conditions of the electric circuit be the same in all cases.

In conclusion, I wish to acknowledge my indebtedness to the Rumford Committee of the American Academy of Arts and Sciences (to which this paper has been presented) for the two grants which made this work possible, and to Professors Hale and Frost of the Yerkes Observatory, for the loan of the grating used. I am also under obligation to Dr. George S. Isham for the gift of the filar micrometer, and to Mr. Cornelius Vanderbilt for generous financial aid. My assistants, Messrs. Hartley and Miller, have proved invaluable at various times; and to Mr. Boord I am indebted for the analysis of the brass wire.

WABASH COLLEGE, CRAWFORDSVILLE, INDIANA,
June, 1905

[1] The statement is made that the spark was 8 or 9 mm in length and its image on the slit about 6 mm.

VARIATION OF ARC SPECTRA WITH PHASE OF THE CURRENT PRODUCING THEM[1]

By H. CREW and B. J. SPENCE

In 1897 the radiation of an alternating current arc in which the current was interrupted during alternate half-periods was examined with some care by Professor Basquin[2] and one of us. The intervals during which the current was cut off were utilized for the examination of the arc. This was accomplished by means of an occulting screen such as that employed by Fleming and Petavel[3] for comparing, at any instant, the photometric intensity of the arc with its power absorption.

The upshot of this experiment was to show that the characteristic radiation of the arc completely disappears before the arc has been interrupted as long as $\frac{1}{1000}$ second, and that there remains, hovering over the arc-gap, a relatively long-lived, bright, detached cloud. The attempt to photograph the spectrum of this luminous cloud met with inferior success, because we could not operate the large occulting screen and generator in the concave-grating room then at our disposal. Having recently obtained a satisfactory outfit, through the generosity of the Carnegie Institution, the problem was again taken up.

An occulting screen three inches in diameter, rotating on the shaft of a synchronous motor, is provided with four radial slots 2° wide and 180° apart in current phase. The hub of the occulting screen (shown in Fig. 1) can be clamped to the shaft of the motor armature in any desired phase so as to allow one to photograph, or observe the arc visually, in any desired phase, or at least to within 2° on each side of the desired phase. Let us define "zero phase" as that phase of the current in which the characteristic radiation is a

[1] Read before the American Physical Society, April 21, 1905.

[2] *Proc. Amer. Acad.*, **32**, 1898.

[3] *Phil. Mag.*, **41**, 315–360, 1896. The work of Fleming and Petavel was not mentioned in our previous paper, because we did not then know of it.

minimum. Then, when the ordinary continuous alternating arc is observed at zero phase, it is at once evident to the eye that nothing remains of the arc except the detached luminous cloud mentioned above. It is not, therefore, necessary to use the intermittent current previously employed; for, without this, the continuous alternating current will give practically every phase of the arc from complete interruption to maximum radiation.

FIG. 1.

In the meantime (i. e., between these two experiments) de Wattville[1] has suggested a very clever modification of this occulting screen which will allow one to photograph several phases of a spectrum at once; but practically his device is limited to non-astigmatic spectrographs and to sources in which the spectrum does not appreciably vary from one part of the source to another.

He employs a screen, shown in Fig. 2, provided with several slots, each at a different phase and each at a different distance from the axis of rotation, thus obtaining on a single photographic plate and in immediate juxtaposition as many phases as there are slots; de Wattville has commented upon the similarity between flame spectra and the spectrum of the arc at zero phase; he has also noted some interesting differences between the behavior of banded and linear spectra.

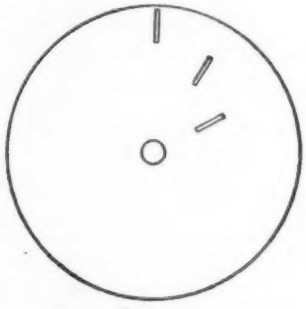


FIG. 2

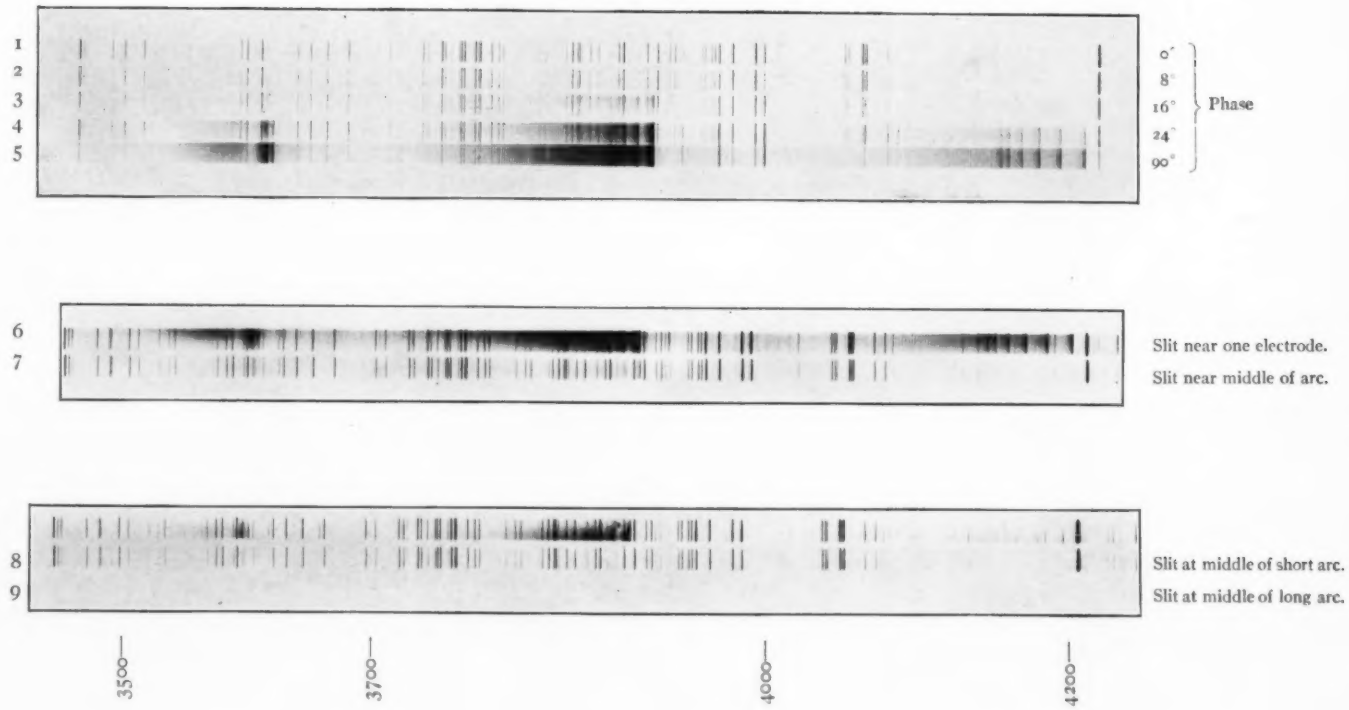
The results which we here describe are, so far as we know, new and additional to those of de Wattville; part of them were obtained before the appearance of his paper. Incidentally we have verified all the results which de Wattville describes.

ORDINARY CARBON ARC

1. The profound difference between the spectra of carbon at maximum and at zero phases is illustrated by the strips numbered 1 and 5 on Plate VII. It is the difference between something and

[1] *Comptes Rendus*, **138**, 485–486, 1904.

PLATE VII


1
2
3
4
5
0°
8°
16°
24°
90°
Phase
6
7
Slit near one electrode.
Slit near middle of arc.
8
9
Slit at middle of short arc.
Slit at middle of long arc.
3500
3700
4000
4200

nothing. In the first strip, which was taken when the current was at zero phase, there is not a trace of any carbon band remaining; in the fifth strip, taken at a phase of 90° (maximum) we have the fully developed carbon spectrum. It is an easy matter to obtain as many phases as one may desire between these two extremes.

2. Near zero phase the cyanogen bands are very weak at the middle of the arc, and comparatively strong near the electrodes. This effect is shown in strips 6 and 7, Plate VII. As a result of this, we observe that when a very short arc, say less than 2 mm, is used the cyanogen bands are present at all phases; but when a long arc, say 6 mm or over, is employed, the cyanogen flutings entirely disappear from the middle of the arc. This phenomenon is illustrated in strips 8 and 9 of Plate VII.

3. At zero phase, the aluminium bands, as well as the *Al* pair between H and K, have their relative intensity greatly increased. In fact, the aluminium flutings appear under precisely the conditions which cause the cyanogen flutings to disappear. Is this in some way connected with the fact that cyanogen is strongly endothermic, and accordingly very stable at high temperatures; while aluminium oxide is fiercely exothermic, and therefore especially stable at low temperatures?

The different behavior of two impurities under identical conditions is well illustrated by the *Al* pair between the *Ca* pair at H and K. The *Ca* pair diminishes in intensity with the phase; and so does the *Al* pair, but at a rate so much slower than that of the *Ca* that one could easily tell from the relative intensity of the two pairs whether the photograph was taken near the phase of maximum or near that of minimum current. This is quite in harmony with the enormous intensity of H and K at the temperature of the Sun; for increase of phase in these experiments undoubtedly means increase of temperature.

4. Between the *K* pair at λ 4044 and λ 4047 lies an iron line whose intensity diminishes with phase so much more rapidly than that of the *K* pair that almost anyone could estimate, from their relative intensities, within 10° of the phase at which the photograph was made. This triplet behaves in precisely the same manner in

the *spark*[1] passed between *heated* carbon electrodes whose temperature is falling.

GRAPHITE ARC

We have not been able to work the graphite arc with alternating currents in the ordinary way. In any event, fifty amperes on a 110-volt circuit does not maintain an arc of sufficient length to project upon the slit of the spectrograph. Accordingly, we used a rotating graphite electrode and found that, at zero phase, the arc gives practically zero light, the only lines appearing between λ 3700 and λ 5300 being three calcium lines, a trace of the cyanogen band at λ 3883, and traces of a few ultra-violet iron lines.

To digress for a moment: A very curious thing happens with this rotating graphite arc, viz., the appearance of the well-known carbon spark line at λ 4267, a *Hauptlinie* of Eder and Valenta. Few lines are so sensitive to rapid changes in E. M. F. This line is strong in the spark between amorphous carbon poles, *provided they are cold;* if, however, the carbon tips have been heated, the line disappears even from a powerful transformer spark. On the other hand, this line which does not appear in the ordinary carbon arc comes out strong in the rotating graphite arc, where the natural quickness of break which characterizes the graphite arc is accentuated by the motion of the electrode. In other words, this line disappears with great readiness from its normal source (the carbon spark) and appears, on slight provocation, in a source where it might be least expected, the graphite arc. Such capricious behavior might lead one to hesitate in assigning this line to carbon; but its presence in pure Acheson graphite leaves little room for doubt as to its identity.

Returning from this digression, it is, perhaps, worth noting that, as the phase increases, all the carbon flutings appear *on the photographic plate* in the order of their intensities, which would be exactly the case if they all made their actual appearance at the same instant. If, therefore, the so-called cyanogen bands are due to a radiating system different from that which emits the so-called carbon bands, the evidence here furnished would seem to indicate, at least, that both systems radiate under similar conditions.

[1] Crew and Baker, *Proc. Amer. Acad.*, **38**, 397–406, 1902.

ALUMINIUM ARC

At zero phase nothing is left of the *line* spectrum of aluminium except the pair between H and K; even an hour and a half's exposure does not leave the slightest trace of the heavy pair at λ 3100. The manner in which the *Al* flutings are intensified, at zero phase, as compared with the line spectrum, has already been mentioned.

Arons and Hemsalech appear to have proved (by working the *Al* arc and spark in hydrogen) that these flutings are due, not to an oxide, but to metallic aluminium. If this be true, the opposite behavior of these two spectra—banded and linear—would seem to indicate that they originate, at least, in a different vibrating system.

MAGNESIUM ARC

The effect at zero phase of magnesium in cored carbons is merely to introduce the *Mg* fluting at λ 5007, or at least to make it visible by removing the green carbon fluting. The *Mg* arc between moving metallic electrodes is reduced to the fluting at λ 5007 and traces of the stronger lines.

IRON ARC

The iron spectrum at zero phase consists essentially of about 300 lines lying between λ 2700 and λ 5600. It is distinctly shorter than the flame spectrum of iron which de Wattville[1] has measured up to λ 2300. On the other hand, it is distinctly longer toward the blue, and therefore presumably hotter, than the spectrum obtained by A. S. King[2] in his electric oven. And what is true of the iron spectrum is probably true of most of the metallic elements, namely, the spectrum of the arc at zero phase is essentially a flame spectrum. The upshot of the whole matter is, therefore, that an occulting screen on the shaft of an alternating generator or on a synchronous motor furnishes a means of obtaining flame spectra, and all the intermediate steps between flame spectra and arc spectra.

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Evanston, Ill.

[1] *Phil. Trans.*, **204**, 139–168, 1905.

[2] *Annalen der Physik*, **16**, 360–381, 1905.

ON THE SPECTRUM OF THE SPONTANEOUS LUMINOUS RADIATION OF RADIUM

PART III. RADIATION IN HYDROGEN[1]

By SIR WILLIAM HUGGINS and LADY HUGGINS

As soon as we found[2] that the glow of radium bromide consisted mainly of light from nitrogen stimulated into luminosity by the radium, and giving the negative-pole spectrum, we formed the intention of photographing the spectrum of the glow when the radium bromide was placed in an atmosphere of hydrogen, primarily in the hope of finding an answer to the question raised in our first paper, "Have we to do with occluded or with atmospheric nitrogen?" and, in the second place, to determine whether the radium is able to render hydrogen luminous.

In these experiments some unexpected results came out, which made it desirable to repeat them many times. This circumstance, together with the long exposures necessary—from ten to fourteen days—and the slow changes which we found to take place in the radium when allowed to remain in the hydrogen for long periods, reckoned in months, have necessarily delayed the publication of this paper. The investigation is still in progress, but it seems desirable not to delay any longer the publication of the results which have been already obtained.

An account of each experiment would make the paper long and unnecessarily tedious. It will be sufficient to give the results of each group of experiments made under similar conditions. The same form of apparatus was used for all the experiments. Small glass vessels were prepared consisting of a round cell with flat base, to hold the radium, into which on opposite sides, tubes of small bore were blown. The walls of the cell were ground flat on the top to receive a thin microscopic cover-glass, or a thin plate of quartz,

[1] From advance proofs of an article to appear in the *Proceedings of the Royal Society*.

[2] *Astrophysical Journal*, **18**, 151, 390, 1903; *Proc. R. S.*, **72**, 196, 409, 1903.

which was cemented down after the radium salt had been placed on an ebonite support within the cell.

As the glow of radium takes place at atmospheric pressure, it was desirable that experiments with hydrogen should be made under like conditions. A current of hydrogen was allowed to flow through the glass vessel for some minutes, until all traces of air must have been carried out; the two tubes were then sealed up, leaving the radium in hydrogen at atmospheric pressure.

Afterwards, a second series of experiments was made with hydrogen at reduced pressure. The glass vessel was connected with a vacuum pump and exhausted to below 1 mm of mercury; hydrogen was then allowed to enter. The vessel was again exhausted and refilled with hydrogen several times, and was then sealed up with the contained hydrogen at the pressure of about 1 mm of mercury.

Portions of the same two specimens of radium bromide which had been used in our former experiments were employed, namely one from Buchler & Co., Brunswick, and the other from the Société Centrale de Produits Chimiques, Paris.

The results of repeated experiments made in hydrogen at atmospheric, and also at reduced, pressure, and with both samples of radium bromide, were uniformly similar. The glow became sensibly fainter to the eye when the radium had remained for a few days in hydrogen; perhaps the diminution of the brightness took place sooner in hydrogen at reduced pressure. Photographic plates, exposed in the spectroscope to radium in hydrogen for the same time as to the same radium in air, showed a feebler spectrum, which was that of nitrogen without any traces of the lines of hydrogen.

From these experiments we may assume either the existence of occluded or combined nitrogen, or that the spectrum was due to minute traces of air which had remained within the vessel. If, however, the increasing feebleness of the glow was due to the latter cause, we should expect that, on unsealing the tubes and admitting air, the glow would at once recover its original brightness. On April 15, 1904, when the radium had remained twenty-six days immersed in hydrogen, the tubes were opened and air blown through, but no recovery of brightness, as estimated by the eye, took place at the time. Then a photographic plate was exposed in the spec-

troscope for seven days, on which, when developed, the nitrogen spectrum was even feebler than on a similar plate which had been exposed for the same time before the air was admitted. A few days later, however, a small increase of brightness was detected by the eye, which continued until the radium slowly recovered its original brightness.

When radium was allowed to remain for months in an atmosphere of hydrogen some unlooked-for results were observed.

Experiment 1.—A portion of the Brunswick radium bromide was sealed up in hydrogen at reduced pressure on June 24, 1904.

a) As in former experiments, the brightness of the radium bromide, as estimated by the eye, gradually diminished.

b) Photographs taken with the spectroscope showed the nitrogen spectrum only, and with increasing feebleness as time went on, until the strongest of the nitrogen bands only were just suspected upon the plate.

c) Some days after sealing up in hydrogen, the radium bromide, which was originally of a yellowish-cream color, began slowly to become darker, until by August 9, 1904, it had reached a dark russet-brown.

d) On March 17, 1905, the radium, which had now been sealed up for nearly eight months, was observed by eye to have become much brighter, indeed nearly as bright as the French radium which had remained in air. Unfortunately, the radium in the hydrogen had slipped out of the ebonite support to the bottom of the cell, and therefore could not be brought before the slit of the spectroscope. It was then decided to open the vessel and remove the radium in order to photograph its spectrum while in this brighter condition. Before placing it in the spectroscope it was thought desirable to compare it again at night, about eight hours after it was taken out of hydrogen, with radium which had remained in air. To our great surprise, the radium removed from the hydrogen had completely lost its light; it was now quite dark, without any sensible glow. It retained its very dark russet-brown color.

e) Before the vessel was opened, while the radium was bright in hydrogen, its radio-active power was measured with an electroscope; after it had been removed from the hydrogen and had become dark

and glowless, its radio-activity was again measured. The amount of the induced leak of the charged leaves was found to be the same as before, showing that the sudden change from a bright condition to one without any sensible glow had not been accompanied by an alteration in the intensity of the β- and γ-rays.

f) The glowless radium was examined in the dark at intervals of a few days. By May 9, 1905, a very faint glowing was perceived, and at the same time the dark brown color was observed to have become less intense. These changes proceeded slowly until, by August 13, the radium had regained its original creamy color and nearly its original brightness. During these three months its radiation, as measured by the electroscope, remained the same.

Experiment 2.—Fortunately, we have for the purpose of comparison a portion of the French radium which has been sealed up in hydrogen at reduced pressure since September 12, 1904, about eleven months. Many photographs of the spectrum of this sealed-up radium have been taken at intervals from last September to the present time, showing with similar exposures, increasing feebleness, but always, when any action could be detected upon the plate, some of the stronger bands of the nitrogen spectrum. Recently, however, a band has appeared in the green part of the spectrum, for which the plate is but feebly sensitive, without any action being discernible on the plate in the blue and violet regions, for which the photographic film is greatly more sensitive. Fortunately, on one plate the chief bands of the nitrogen spectrum, though excessively faint, can be just detected, while at the same time the new band, falling in a much less sensitive region photographically, is relatively strong.

The defined line which begins the band on the less refrangible side is a little more refrangible than the brighter edge of the green band of the Swan spectrum at λ 5165. The band has not yet been identified.

The band is accompanied by a faint continuous spectrum which runs back to D.

The radium bromide has turned to a dark russet-brown color, as in the former experiment. To the eye the brightness of the radium has remained greatly diminished, until within the last few days, when we suspect that, as in the preceding experiment, a slow increase of brightness has set in.

On re-examining all the photographs of the spectrum of the glow of radium which we have taken, a plate was found, developed on August 23, 1904, of the spectrum of a portion of the French radium which had been sealed up in hydrogen for a few days only, but when its light had faded to about one-half, which shows very faintly, but unmistakably, the new band.

The suggestion presents itself to the mind whether in both experiments, when the radium had almost ceased to glow with nitrogen light, it was able to stimulate the molecules of the substance producing the band into luminescence. On this supposition an explanation of the sudden going out of the bright glow, when the radium was taken out of the atmosphere of hydrogen in the first experiment, would be found in the absence in the atmosphere of the needful molecules, the radium having lost temporarily the power of exciting nitrogen; unless we take the view that, to be stimulated into luminescence, nitrogen must be not merely in outward contact with the radium, but in a more intimate connection with it, which time is required to bring about.

The suggestion was considered in a former paper whether the operative cause of the glow was to be found in the β-rays, which are known to be analogous to the cathode corpuscles, upon the nitrogen of the air. In these recent experiments the electroscope showed that these rays, and the γ-rays, were being radiated with undiminished force at the time that the radium bromide remained glowless in air. It may be mentioned here that the cathode discharge is efficient in bringing out easily the first spectrum of hydrogen,[1] but the radiations of radium appear to be unable to do this. Our experiments seem rather to support the view, suggested in our first paper, that the spontaneous light of radium may not be produced by any form of its radiations acting upon the nitrogen of the air outside it, but by a more direct action through encounters with nitrogen molecules, in some way associated with the radium, of those molecules of the radium which are undergoing active changes.

[1] Liveing, *Proc. Camb. Phil. Soc.*, **12**, 338, 1904.

AN OBSERVATION OF THE ZODIACAL LIGHT TO THE NORTH OF THE SUN

By SIMON NEWCOMB

The zodiacal light is commonly conceived and described as a phenomenon extending on both sides of the Sun, in or near the plane of the ecliptic, its possible breadth being left out of consideration, except as implied in the term "lens-shaped," sometimes used to designate its form. So far as I am aware, the possible thickness of the lens has never been considered, nor do I know of any attempt having been made to see the light north or south of the Sun. Such an observation, being necessary to the delineation of its complete outline, must be regarded as of prime importance in defining the zodiacal light itself. The observation requires a rare combination of conditions. One of these is that the Sun shall be more than 18° below the horizon at midnight, but as little in excess of 18° as practicable. The obvious reason for this is that, at a less depression than this, there might be some effect of twilight; while the farther it is below the limit, the less likely is the light to be seen. In fact, unless the minor semi-axis of the light considerably exceeds 18°, it may be forever impossible to distinguish it from twilight itself. The condition in question can be fulfilled only in middle or northern latitudes, not much south of 46°.

The other condition is that the observation shall be made from as elevated a point as possible above the Earth's surface. The light is known to be so susceptible to atmospheric absorption at ordinary elevations, and near the horizon, that we never see it, even in the zodiac, during those summer months when the ecliptic makes a small angle with the horizon. To what height it might be necessary to ascend could be determined only by trial.

During several summers past I have endeavored to make this observation from elevated points in the White Mountains and elsewhere, but have invariably been defeated by the density, smokiness, or haziness of the air near the horizon. Better stations could doubtless be found on the mountains of the Pacific coast; but not having

visited that region, Switzerland seemed to be the next best attainable region. It happens, however, that even in that country nearly every point of observation which could be readily occupied throughout the night has yet more elevated points to the north of it, which cut off the view of the horizon. This is particularly the case at the Gorner Grat and the base of the Matterhorn, which have respectively the Mischabel and the Weisshorn to the south of them.

Speaking of the problem to Dr. Frei, a Swiss geographer, he suggested that the Brienzer Rothorn, a mountain to the north of Lake Brienz, might be a suitable and practicable point, and was accessible by a rack-and-pinion railway. The position is: $\phi = 46° 47'$; $\lambda = 8° 3'$ E. The observation would be much facilitated by the existence of a hotel only three hundred feet below the summit of the mountain. The altitude of the mountain, 7700 feet, although not as great as could be desired, yet seemed to offer sufficient hope of success to justify a trial; I therefore repaired thither on July 26, 1905.

The condition of the air on Lake Lucerne, as I passed over it, was far from satisfactory, it being more hazy than I had ever before seen it. On reaching the mountain, it was found that the upper surface of the haze was slightly below the astronomical horizon, so that the prospects for an observation, if the light were bright enough to be seen, were fairly good. The following are notes of the observations made during my stay on the mountain:

Wednesday, July 26, 1905.—Reached Hotel Rothornkulm at $6^h\ 45^m$, Local M. T., about an hour before sunset. Air below very hazy. Sharply defined upper surface of denser yellow haze well marked, about 10′ or 15′ below the astronomical horizon; but air not satisfactorily transparent, even above this line, as shown by the Sun's lurid color in approaching the haze. On entering the latter it turned deep red, and became quite invisible before reaching the sensible horizon marked out by the distant mountain tops.

$9^h\ 0^m$. Still bright twilight.

10^h. Twilight seems to have completely passed. A faint glow is now visible over the northwestern horizon, the central part of which is on a vertical passing near or between the pointers, and which can be traced about 12° or more on each side of the central region.

$10^h\ 20^m$. The central part of the glow now seems to be north of the vertical through α *Ursae Majoris*, and about 20° west of north.

$11^h\ 30^m$. Now see that above the western horizon, and below *Arcturus*,

the sky is whiter than elsewhere at the same altitude. Sweeping the eye around the horizon, a well-marked glow is visible through the entire quadrant from north to west, rising to an altitude of some 20° or 25°.

About midnight.—Looking out of the window toward north, a slight glow visible around the northern horizon does not seem to be anything more than a phenomenon generally visible during the night at any point. The haze seems to have so thickened as to preclude any decision as to what is actually to be seen.

July 27 and 28.—Bad weather.

July 29.—Enveloped in cloud most of the day and early evening.

Midnight.—Quite clear, except a stratum of cloudlike haze around the northern horizon, with thick atmosphere immediately above it. But an illumination of the low northern sky is distinctly visible from north window, and observations from the Kulm are decided upon.

$11^h\ 45^m$ to $12^h\ 5^m$ M. T. While walking up to the Kulm, from occasional glimpses, the maximum of brightness seemed to be just below *Capella*.

$12^h\ 5^m$ to $12^h\ 10^m$ M. T. The characteristic zodiacal glow distinct and unmistakable—not so bright as ordinarily seen east or west of the Sun, yet several grades brighter than the limit of doubt. It extends from a little east of *Capella* to a region below the pointers. The maximum of brightness is midway between *Capella* and the north point, say between 10° and 15° east azimuth, and at 10° of altitude. The appearance of maximum brightness below *Capella* was evidently due to the Milky Way.

$12^h\ 45^m$ M. T. Carefully scanning the sky, especially in the region of the ecliptic and the horizon. At one time had a strong impression of an illumination in the south much fainter than that in the north; but this was supposed to be the Gegenschein, fainter and more diffused than I had ever before seen it. But the impression itself soon faded, though the sky seemed in the minutest degree brighter in the low south than elsewhere at the same altitude.

I now see that the glow in the north does not extend quite to the vertical of the pointers. South of *Capella* it is merged in the Milky Way. Tried to trace it between the *Pleiades*, now well above the horizon, and *Aldebaran*, now upon it, but the presence of *Jupiter* rendered a conclusion impossible, though a glow was well seen. Farther south no trace could be made out.

Later.—*Venus* was seen rising toward the horizon; and it was evident that the combination of its light with that of *Jupiter* would prevent any positive ascertainment of the continuity of the glow in the north with the familiar phenomenon of the zodiacal light.

The conclusion reached from these observations is that, in the direction of the Sun's axis, the zodiacal light is bright enough to be plainly seen to a distance of about 35° on each side of the Sun. Of course, the actual observations are made only on the north side, but there can be no reasonable doubt of the symmetrical character of

the phenomenon. This conclusion is quite in accord with what anyone may notice who carefully examines the light, as near the horizon as it can be seen, on a suitable evening in February, or a morning of October. It will then be seen that the light rapidly broadens toward the horizon, although, in our latitudes, it cannot be seen near the actual horizon, owing to atmospheric absorption. Of course the boundary of the phenomenon does not admit of precise delineation, the fading off being gradual and continuous. The limit of 35° which I have set, nevertheless seems to me much more precise than any limit that has been, or can be, set in the plane of the ecliptic.

I suggest that the zodiacal light be hereafter described as a luminosity surrounding the Sun on all sides, of which the boundary is nowhere less than 35° from the Sun, and which is greatly elongated in the direction of the ecliptic.

I may also invite attention to my inability to see the Gegenschein under circumstances on the whole quite favorable. The depression of the Sun was 25°, and the conditions seemed extremely favorable.

SPECTROGRAPHIC OBSERVATIONS OF CERTAIN VARIABLE STARS

By EDWIN B. FROST

During the past year I have added to the observing program for the Bruce spectrograph a few variable stars (chiefly of the *Algol* type) for which a knowledge of the radial velocity would be likely to be of interest. Most of these stars are faint, and in many instances the spectral lines are so diffuse that the plates are not susceptible of precise measurement. All of the plates were obtained with a dispersion of one prism and with a camera of 607 mm focus. The results as here communicated are to be regarded only as provisional, but the measures clearly show that *R Canis Majoris*, *Z Herculis*, and *U Sagittae* are spectroscopic binaries, and their velocities correspond in sense to what would be expected from the phase in the light-variation. On account of the long exposures required the accumulation of plates in a season is necessarily small.

I have added to the usual data the length of exposure, since it constitutes in some cases quite a fraction of the half of the star's period of variation. The interval after (*a*) or before (*b*) the nearest minimum is also given. The times of minimum are taken from Hartwig's ephemerides in the *Vierteljahrsschrift der Astronomischen Gesellschaft.*

R Canis Majoris ($\alpha = 7^h\ 15^m$; $\delta = -16°\ 12'$; Mag. = 5.9 to 6.7)

Plate	Date	G. M. T.	Exposure	No. Lines	Radial Velocity	Interval from Minimum
IB 492....	1905 January 27	$17^h\ 5^m$	80^m	4	-66^{km}	*a* $6^h\ 30^m$
499....	February 3	17 12	80	7	−54	*a* 11 2
530....	April 7	13 59	70	3	−13	*b* 6 54

The period of the star's light-variation is $27^h\ 16^m$, with which the above observations would suggest an orbital velocity of about 27 km and a velocity of translation of the system of about −40 km, a circular orbit being assumed.

The spectrum is between Vogel's types Ia2 and Ia3, but the

lines are rather broad. Those best for measurement are at λ 4481 and λ 4549 and they receive the greatest weight. The three plates so far obtained do not show indications of the spectrum of the second star.

Z Herculis ($\alpha = 17^h\ 54^m$; $\delta = +15°\ 9'$; Mag.=7.0 to 8.0)

Plate	Date	G. M. T.	Exposure	No. Lines	Radial Velocity	Interval from Minimum
IB 543....	1905 June 2	$16^h\ 36^m$	200^m	4	-75^{km}	*a* $10^h\ 19^m$
551....	June 19	17 31	300	–	—	*a* 35 56
578....	August 28	15 51	230	3	− 2	*b* 10 37

The double period of light-variation is $95^h\ 50^m$ (from even to even minimum, from which the above intervals are reckoned). Plate 551 was taken through a thick sky and is hardly measurable. It clearly indicates a moderate negative velocity, however, in accordance with the direction to be expected. A negative velocity of the system is implied by the fact that the last value is slightly negative. The velocities given may be regarded as uncertain by several kilometers.

The spectrum is much like that of the preceding star, but the lines are all faint, as none of the plates is fully exposed. Except for a faint suggestion of duplicity at *Hγ* on the last plate, there is no indication of the spectrum of the second component.

U Sagittae ($\alpha = 19^h\ 14^m$; $\delta = +19°\ 26'$; Mag.=6.5 to 9.1)

Plate	Date	G. M. T.	Exposure	No. Lines	Radial Velocity	Interval from Minimum
IB 542....	1905 May 19	$20\ 6^m$	160^m	3	-49^{km}	*a* $17^h\ 56^m$
555....	July 14	16 39	199	2	+69	*b* 20 52

The star's period of light-variation is $3^d\ 9^h\ 8^m$.

I have to thank Mr. Barrett for taking the second plate. The spectrum is of the *Orion* type, and the measurements depend chiefly on the helium line at λ 4472, and the magnesium line at λ 4481. Evidences of lines due to the second star do not appear on these plates.

U Ophiuchi.—Of this well-known *Algol* variable I have obtained five spectrograms, three in 1904 and two in this season. The spectrum is of the *Orion* type, but the helium lines are faint. The

hydrogen lines and λ 4481 are exceedingly broad and diffuse, and the measurement of the plates will be very difficult. There are strong indications of two component spectra, and there are evidently variable displacements of the lines, but a definite statement of results must be deferred until measurements can be made on the best of these plates and on others yet to be secured.

RX Herculis.—Two spectrograms have been secured this season of this star (magnitude 7.0 to 7.8), but the diffuseness of the lines almost prohibits measurements. The helium lines are probably present but exceedingly faint. On the first plate λ 4481 seems widely double, but single on the second. Enhanced lines of titanium and iron are present but faint. Their displacements are appreciably different on the two plates. We hope to secure another spectrogram this season.

Y Cygni.—I obtained a first plate of this star on July 19, 1904, with an exposure of 202 minutes; and a second on June 12, 1905, in 183 minutes, with the assistance of Mr. Barrett. The helium lines are very faintly represented. The appearance of $H\gamma$ is peculiar, and its duplicity is suggested on the first plate, unless an enhanced titanium line at λ 4338 is unduly strong. The lines are diffuse, and quantitative results probably cannot be derived from the first plate. Definite statements as to the star's radial velocity are reserved until other plates can be obtained and measured.

R Coronae.—In view of the interest that the recent light-variations of this long-period variable have aroused, it may be well to communicate the measurements of two plates I have secured of this spectrographically faint object.

R Coronae ($\alpha = 15^h\ 44^m$; $\delta = +28°\ 28'$; Mag.=5.5 to 10)

Plate	Date	G. M. T.	Exposure	No. Lines	Radial Velocity
I B 58.....	1903 July 24	$17^h\ 5^m$	240^m	11	$+14^{km}$
575.....	1905 August 25	15 38	200	11	+13
				Mean	+13

Three days after the first date the magnitude was measured by Mr. Parkhurst and found to be 7.2. On the second date he estimated the magnitude to be rather brighter than 7.

A very wide slit (0.1 mm) was necessary on account of the star's

faintness, and the lines in the stellar and comparison spectrum are broad. The second spectrogram is the better, but neither can be measured with much refinement, although settings can be made on numerous lines, as the spectrum is of the early solar type like that of *a Persei*. In view of the difficulty of measurement, the two results may be considered in accidentally close agreement, and as indicating no change in the star's radial velocity. The above values were derived from the eleven best-defined lines measured on both plates.

No qualitative change can be detected in the appearance of the spectrum on the two plates.

I acknowledge with pleasure the assistance received during these rather tedious exposures from Mr. Sullivan, who as usual shared in all the guiding.

YERKES OBSERVATORY,
September 16, 1905.

THE EFFECT OF A PRESSURE OF 37 ATMOSPHERES ON CERTAIN LINES OF THE ARC SPECTRUM OF IRON

By W. J. HUMPHREYS

After several years of gradual preparation, I recently came into a position to continue experimental work on the effect of pressure on spectra. The equipment consisted in part of a Rowland concave grating of 14,438 lines per inch (568 per mm) and 21.5 feet (6.5 meters) focal length, with a ruled surface approximately 6×2 inches (15×5 cm), a four-stage Norwalk compressor, and a specially designed large, forged-steel bottle in which an electric arc was inclosed. The excessive difficulty of making this forging caused much delay, as a number of good firms declined undertaking it, and I have to thank Professor Stratton for giving me the address of The Janney Steinmetz Co., of Philadelphia, who furnished a most satisfactory piece of work. The bottle and its attachments will be described in another paper.

After several attempts, a fair negative was secured of a portion of the iron spectrum, due to an electric arc produced by a 220-volt direct current, under a pressure of 37 atmospheres. But at this time the experiments were unavoidably discontinued, and therefore a report on the results obtained is made at once, in the hope that they may be of value to others interested in spectrum work. It is my purpose, however, to take up this investigation again at the earliest practicable date.

An inspection of this negative showed at once that the tendency to reverse is much greater under a pressure of 37 atmospheres than it is under that of only one. Many lines at this high pressure spread out considerably, while others, like λ 4315.21 and λ 4337.14, remain fairly clean and sharp. Several, of which λ 4233.76 and λ 4236.09 are good examples, become somewhat hazy and show large displacements toward the red end of the spectrum. At least one line, λ 4227.60, not only shows a marked shift, but also indicates its gradual disintegration as a line under increasing pressure. It seems as though a somewhat higher pressure than that used would cause it to disappear as a broad faint haze, entirely to the red side of its normal position.

The lines measured are given in the table. The wave-length λ is taken from values given by Kayser and Runge. Under "intensity and character" R means reversed, while the numbers show roughly the relative intensities; 1 indicating the faintest line measured, 2 referring to a line about double this intensity, 3 to one of three times that value, and so on for higher numbers. Under Δλ is given in Ångström units the observed increase in wave-length of the respective lines as formed under a pressure of 37 atmospheres over their values when formed under that of but one atmosphere.

On the whole, these values of Δλ are reasonably well proportionable to those obtained some years ago,[1] at much lower pressures.

IRON LINES AND THEIR INCREASE IN WAVE-LENGTH (Δλ) PRODUCED BY AN INCREASE IN PRESSURE ABOUT THE ARC FROM 1 TO 37 ATMOSPHERES

Wave-Length λ	Intensity and Character at 37 Atmospheres	Intensity and Character at 1 Atmosphere	Δλ	Wave-Length λ	Intensity and Character of 37 Atmospheres	Intensity and Character at 1 Atmosphere	Δλ
3903.06	R 4	1	0.121	4156.88	2	1	0.118
3948.87	2	1	.090	4181.85	2	1	.095
3950.05	2	1	.072	4202.15	R 6	2	.076
3951.25	2	1	.133	4219.47	3	2	.114
3956.77	R 2	R 1	.120	4222.32	2	1	.202
3969.34	R 6	R 2	.114	4227.60*	2	2	.365
3977.83	2	1	.065	4233.76	4	2	.213
3984.08	2	1	.104	4236.09	6	2	.183
3997.49	3	1	.071	4250.28	6	2	.215
3998.16	2	1	.130	4250.93	R 6	2	.076
4005.33	R 6	2	.115	4260.64	R 8	2	.197
4009.80	2	1	.060	4271.93	R 12	R 3	.148
4013.91	2	1	.087	4282.58	3	1	.070
4021.96	2	1	.091	4294.26	4	1	.101
4045.90	R 12	R 4	.131	4299.42	4	2	.193
4062.51	R 10	R 3	.148	4307.96	R 10	R 3	.147
4071.79	R 8	R 2	.161	4315.21	2	1	.070
4118.62	2	1	.093	4325.92	R 12	R 3	.131
4143.96	R 6	R 2	.129	4337.14	2	1	.095
4154.57	2	1	.099				

* The position of this line cannot be determined with much certainty. It is greatly displaced, but very hazy and ill-defined.

The darkest parts of reversals and the most intense portions of the unreversed lines—the positions of the lines when due to a small amount of material—were the places measured. It was impossible, however, to measure them with great accuracy, as they are not

[1] *Astrophysical Journal*, **6**, 200, 1897.

sufficiently narrow and well defined. Therefore, hoping to reduce errors as much as practicable, the settings were made by moving the plate first in one direction, so that increasing scale-numbers meant increasing wave-lengths; and then, after reversing the plate, by moving it in the opposite direction, so that increasing numbers meant decreasing wave-lengths. In all, five measurements were made in each direction, and the values given are the average of these ten reasonably concordant (usually to within 10 per cent. of each other, though sometimes differing by as much as 20 per cent.) separate settings. The dividing engine used for this purpose was one of the large type furnished by the Geneva Society. No corrections were made for the errors of the screw, as the errors of setting did not justify this refinement.

This work was done in the physical laboratory of the University of Virginia, during my connection with that institution, and I wish to thank Professor Smith for his constant support in my efforts to secure the necessary equipment. My thanks are also due the Rumford Committee of the American Academy of Arts and Sciences, one of whose grants materially aided me, particularly in getting the pressure bottle and fitting it to the needs of the investigation.

U. S. Weather Bureau Research Observatory,
Mount Weather, Bluemont, Va.,
September, 1905.

REVERSAL OF BANDS

By W. J. HUMPHREYS

Liveing and Dewar,[1] by feeding a considerable amount of cyanide of titanium to an electric arc surrounded by a magnesium crucible, obtained, of the cyanogen bands, "complete reversals of the five bands near L, of the two strong bands near N, and a less complete reversal of the six bands beginning at about λ 4215." Negative results were given by the cyanides of other metals.

Petavel and Hutton,[2] while working with an electric arc between carbons surrounded by air at a pressure of forty atmospheres, secured a "marked reversal of the five heads of the cyanogen band beginning at λ 3883."

The heads of this band are also nicely reversed on one of my own negatives, recently taken under a pressure of only sixteen atmospheres.

The conditions in each of the above were unusual, and it may therefore be worth while to call attention to a number of bands that reverse, more or less easily, in the open arc at atmospheric pressure. A direct 110-volt current was used, and the light analyzed by a Rowland concave grating of 6.5 meters focal length. The lower or positive carbon was bored axially and filled with the substance under examination.

The most easily obtained and the most beautiful of these reversing bands are two due to the fluoride of calcium, one at about λ 6030, the other about λ 6060. Their reversal is an unusually beautiful spectroscopic phenomenon. On watching one may see them, as the quantity of material in the arc fluctuates, suddenly seem to head first one way and then the other; indicating a reversal, not only of the heads themselves, but also of many of the next succeeding fine lines; with, however, a gradual falling off in the distinctness of the reversal, and finally only an increasing intensification of the more and more distant and ordinarily fainter tail lines. However, as these lines are exceedingly close together, it is not easy to deter-

[1] *Proc. R. S.*, **33**, 3–4, 1881. [2] *Phil. Mag.*, (6) **6**, 571, 1903.

mine what happens to them individually; only the general effect is conspicuous.

The fluoride of strontium gave five reversed bands, and the fluoride of barium two. All these are listed in the subjoined table, in which the wave-length does not refer to any one of the heads, but only to that place in the spectrum about which the band in question occurs; the object of the table is simply to locate each band as a whole.

The positions of the heads of bands due to the fluorides of calcium, strontium, and barium have been given by Fabry.[1]

REVERSING BANDS

Substance	λ
Calcium fluoride (CaF_2)	6030
Calcium fluoride (CaF_2)	6060
Strontium fluoride (SrF_2)	5780
Strontium fluoride (SrF_2)	6300
Strontium fluoride (SrF_2)	6400
Strontium fluoride (SrF_2)	6515
Strontium fluoride (SrF_2)	6625
Barium fluoride (BaF_2)	4960
Barium fluoride (BaF_2)	5015

This work, by the kindness of Professor Smith, for which I wish to thank him, was done in the physical laboratory of the University of Virginia.

U. S. Weather Bureau Research Observatory,
Mount Weather, Bluemont, Va.,
August, 1905.

[1] *Journal de Physique*, (4) **4**, 245, 1905.

Minor Contributions and Notes

ON THE ENHANCED SERIES OF TITANIUM, IRON, AND NICKEL

Mr. F. E. Baxandall,[1] replying recently to a paper by the writer[2] on the subject of enhanced lines, has criticised the results reported in that paper. Probably the main difference between Mr. Baxandall and the writer is merely a difference in judgment as to what constitutes enhancement. It was pointed out in the original article that in comparing the relative intensities of arc and spark lines, the question of the proper exposure time assumes the greatest importance and is not easily settled. The Kensington observers, although they recognize the fact that some lines are more enhanced than others, seem to divide all lines into two definite classes, enhanced lines and lines that are not enhanced. Yet Mr. Baxandall expresses surprise that on the author's plates only one iron line was found stronger in the arc than in the spark, plainly implying that this was contrary to his own experience. The inevitable conclusion is that there is room for at least a third class of lines; and, in fact, there are many gradations from the most strongly enhanced lines to those that are just the reverse.

The facts in the case seem to be as follows: The relative intensity of lines in spark spectra is by no means the same as that of the same lines in arc spectra. Moreover, if it is possible to divide these lines into groups such that the relative intensity of all the lines in one group is the same for arc as for spark, but that the relative intensity differs as we pass from one group to another, any such division is extremely rough, and must depend very greatly upon the individual's judgment. Then obviously the only procedure for an investigator is so to choose his exposure times that as many lines as possible shall have about the same intensity in the two cases. Here is where differences in judgment can enter to a great extent, and the difficulty is still further complicated by the fact that an increase or decrease in the exposure times of both spectra (without changing their ratio) can change the contrast between the photographic images of individual lines to a considerable degree. Hence it seems to the author not at all remark-

[1] *Astrophysical Journal*, **21**, 337, 1905. [2] *Ibid.*, **19**, 322, 1904.

able that the results obtained at Kensington should indicate a much smaller number of enhanced lines than he himself found.

A minor point of discussion is the inclusion in the original paper of a number of lines which were marked as of doubtful origin, inasmuch as they could not be identified in any published lists. These Mr. Baxandall rejects as probably due to impurities, but it is the belief of the author (and also of Director E. B. Frost, at whose request the work was done) that most of them belong to the elements under consideration, since every effort to identify them with other elements was unsuccessful.

There remain a few cases where lines listed as enhanced in the Kensington publications were not found to be so by the writer. It is difficult to account for these cases, except on the assumption that the difference was caused by the different conditions under which the spark was generated.

H. M. REESE.

UNIVERSITY OF MISSOURI,
September 8, 1905.

Reviews

The Dynamical Theory of Gases. By J. H. Jeans. Cambridge: University Press; New York: The Macmillan Co., 1904. Pp. viii + 352. $4.50, net.

In this work the writer aims "to develop the theory of gases upon as exact a mathematical basis as possible." The introductory chapter is followed by a discussion of "The Law of Distribution of Velocities," including also the H-theorem, and Boltzmann's theorem of the equipartition of energy. Then follow two chapters on the "Physical Properties of a Gas," including Boyle's law, van der Waals's equation, the virial of Clausius, and entropy and its interpretation. These are followed by chapters on a "Non-Conservative Gas," discussing the problem of the dissipation of the energy of a gas by radiation in the ether, and an attempt to co-ordinate the kinetic theory with the known facts regarding gaseous spectra, and the obvious deductions therefrom regarding molecular and atomic vibrations. Six chapters treat of viscosity, and other phenomena depending on mean free path, Maxwell's assumption of a repellent force between molecules varying as the inverse fifth-power, and the propagation of sound. Of the remaining chapters, XVII deals with "Planetary Atmospheres;" XVIII, with "Molecular Aggregation and Dissociation;" and XIX, with "Numerical Values," especially as to the size of molecules.

In an atmosphere, subject like ours to rapid disturbances, the conditions of pressure and temperature approximate more nearly to convective (adiabatic) than to isothermal equilibrium. The temperature is then a linear function of the vertical height, and the formula suggests a free surface at the height of about 29 kilometers (18 miles). The composition of such an atmosphere is uniform. The outer atmosphere must consist of molecules so scattered that there are practically no collisions, but describing orbits under the Earth's gravitation. Because of the lack of collisions, its condition is most aptly described by the equations for isothermal equilibrium, which simply deal with the averages of a fortuitous aggregation. The constitution is no longer uniform, but the lighter gases must be present in relatively greater proportions. The assumption of the temperature of 17°, absolute, for the outer atmosphere, leads to the conclu-

sion that beyond a distance of 80 kilometers (50 miles) from the Earth's surface the atmosphere must be pure hydrogen.

The author deduces the formulæ connecting the escape of molecules from the outer atmosphere with the temperature and gravitational constant. In the case of the Earth, at any temperature of the outer atmosphere less than 230° C. the loss of atmosphere is inappreciable, much more so at actual temperatures; while, if this temperature was ever greater than 740° C., the hydrogen atmosphere must then have been lost, and the hydrogen of the present atmosphere is to be explained as a later addition, probably from chemical action at or near the Earth's surface. The superior planets, except *Mars*, would even more than the Earth be able to retain their atmospheres. From *Mars*, hydrogen and helium would probably escape, while the heavier gases would be retained. For *Venus*, a temperature of 130° C. would be required for the escape of hydrogen, so that she probably retains an atmosphere. *Mercury*, smaller and hotter, is probably devoid of atmosphere, as also the Moon. At the Sun, a temperature of 1,530,000° C. would be required for any appreciable loss, even of hydrogen.

Misprints and minor errors are delightfully few. On p. 118, § 135, is the statement: "Equation (281) admits of negative values for p, whereas an examination of the physical conditions shows that p is necessarily positive." While p is *usually* positive, the negative values given by the equation (that of van der Waals) apply to the *liquid* state, in which negative pressures have been observed, as when the mercury "sticks" to the top of a barometer tube. Also on p. 313, § 375: "When there is no limit to the height of the atmosphere, the neglect of rotation is impossible." In the immediate context it is shown that for distances from the Earth's surface of less than an Earth-radius the rotational effect, even if the Earth is able to drag the air with it, is very small as compared with the gravitational, while the mean free path, at that height, is many times the Earth-radius, so that at greater distances the drag-effect, and hence the rotation, would be practically non-existent.

The book is a piece of conscientious work by one who has already made valuable contributions to this subject. It belongs rather with the classical work of Burbury and of Boltzmann than with any of the attempts at a simple or more popular presentation. Competent criticism of the validity of its conclusions on disputed points can be made only by the greater masters of the subject.

W. P. Boynton.

Atlas of Emission Spectra of Most of the Elements. By A. HAGENBACH and H. KONEN. Authorized English edition by A. S. King. Jena: Fischer; London: Wesley, 1905. Pp. vii+70; with 28 plates. £1 7*s*.

Many volumes dealing with subjects in science are addressed to readers of a particular level. Such a book may be uninteresting to one who has pushed the subject to a higher level on account of its elementary character; at the same time, it may not attract a younger student, because it lies beyond his ken. But this *Atlas of Spectra* is a work which will probably be of interest to all students of spectroscopy, containing as it does much that is easily comprehended by the beginner, and something that will be new to nearly every scholar.

The volume contains no fewer than 280 photogravures exhibiting the spectra of 68 out of a list of 79 elements. With the exception of fluorine, the 11 elements omitted all belong to the class described as "extremely rare;" so that we have before us what is by all odds the most comprehensive survey of spectra ever published.

The scale of the maps is uniform, being approximately 16 Ångström units (tenth-meters) to the millimeter. The sources employed are principally the metallic arc, the carbon arc, the carbon spark, the vacuum tube, and the coalgas-oxygen flame. Each element has been studied under various conditions, so that the maps are well adapted to the illustration of nearly all the principles of modern spectroscopy. Among interesting features of this kind are the differences between arc, spark, and flame spectra, the positive and negative pole spectra of nitrogen, the red and blue spectra of argon, the effect of self-induction, common impurities running through nearly all spectra, the phenomenon of air lines in practically all spark spectra, ghosts, reversals, the distribution of series lines, etc.

The value of the map is greatly enhanced by the accompanying notes which point out the more important lines, their wave-lengths and physical characteristics, the principal impurities, etc. In addition to these details concerning particular spectra, there are ten pages of "Spectroscopic Notes" giving a discussion of laboratory methods and of the variability of spectra. These notes really constitute a summary of present spectroscopic practice.

The authors would perhaps have anticipated some questions if they had told us why they reproduce the positive instead of the negative of their photographs. Black lines on a white ground, as illustrated by the maps of Eder and Valenta and Higgs, would seem to be more easily read.

Possibly the positive was chosen, in the present case, because it brings out the reversals so beautifully.

The clear and idiomatic English of Mr. King adds much to the value of the volume for American readers, and even possibly for that large group of young men who "read German quite as easily as English."

HENRY CREW.

Lehrbuch der Physik. Von O. D. CHWOLSON. Band II. Pp. xxii + 1056. Figs. 658 and 3 stereoscopic pictures. Translated from the Russian into the German by H. PFLAUM. Braunschweig: Vieweg & Sohn, 1904. 18 marks; bound, 20 marks.

The favorable impression which was created by the first volume of this compendious textbook of physics is maintained in the second volume. It treats of sound in eleven chapters, covering 141 pages; while the remaining 900 pages (in eighteen chapters) are devoted to radiant energy, dealing with the subject in a highly satisfactory manner. We may note as special features the great wealth of illustration and the very full references to the literature of the subject, which are collected at the close of each chapter, whence the reader's attention is not diverted by footnotes. The references are given in chronological order for each of the sections into which the chapters are subdivided. Only elementary mathematics are used, but the dimensions of the book would be too greatly extended if much space had also been given to purely theoretical considerations.

After an introductory chapter, the part dealing with radiant energy treats of the transformation of heat energy into radiant energy (under which Kirchhoff's law is discussed), the velocity of propagation, the reflection, refraction, dispersion, transformation, and measurement of radiant energy, in nine chapters. The seventh chapter, treating of dispersion, gives over a hundred pages to what would be included under spectroscopy. The methods and results described are entirely up to date, including in many cases researches as late as 1903. The ground is well covered, although, of course, many important topics receive but limited attention. Celestial spectroscopy is not neglected, occupying more space than might be expected in a work on physics. It is interesting to see the cut of the flash spectrum (Fig. 292) obtained by W. K. Lebedinski at the total eclipse of July 28, 1896, or on the occasion when Schackleton obtained his well-known photograph of the flash spectrum. Gratings and interference methods in spectroscopy receive an excellent discussion in Chapter XIII, on "Interference." The different varieties of photometers are fully dis-

cussed in the ninth chapter, more completely and with better illustrations than in ordinary textbooks on physics. Interesting figures are given of the author's pyrheliometer and actinometer in use in Russian observatories.

Optical instruments are discussed in the tenth chapter, rather in the conventional method. Some points in physiological optics are taken up in the eleventh chapter, and optical phenomena of the atmosphere in the twelfth. The illustrated description of Michelson's work on the comparison of the meter with the wave-length of the three cadmium lines will be welcomed by those who have not the original memoir at hand. The chapters on double refraction and on polarization appear to follow the conventional lines.

On the whole, the volume will be found very useful for reference, as has been confirmed by the reviewer's experience during the several months that it has been at hand. It is likely that it will be of special interest to the readers of this *Journal*, as it deals so largely with radiant energy, and it will presumably be obtained by many who do not feel able to purchase the whole work. E. B. F.